

# THE AMERICAN PSYCHOLOGIST

VOLUME 14



NUMBER 3

*March 1959*

*Published Monthly by*

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# THE AMERICAN PSYCHOLOGIST

Journal of the American Psychological Association, Inc.

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Volume 14

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THE AMERICAN PSYCHOLOGIST is published monthly at Prince and Lemon Streets, Lancaster, Pennsylvania. Subscription: \$8.00, single copy: \$1.00. Communications on editorial and business matters and advertising should be addressed to the American Psychological Association, Inc., 1333 Sixteenth Street N.W., Washington 6, D. C. Address changes must reach the Subscription Office by the 10th of the month to take effect the following month. Undelivered copies resulting from address changes will not be replaced; subscribers should notify the post office that they will guarantee second-class forwarding postage. Other claims for undelivered copies must be made within four months of publication.

Second-class postage paid at Lancaster, Pa.

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# PHILOSOPHICAL EMBARRASMENTS OF PSYCHOLOGY<sup>1</sup>

HERBERT FEIGL

*Minnesota Center for Philosophy of Science*

I APPRECIATE deeply the honor and the privilege of being the guest speaker for this distinguished audience. You chose to invite a philosopher, and I can only hope you will not suffer too much from the consequences.

As I am sure you know, philosophers are specialists in generalities. They are usually also somewhat opinionated. Hence, there is the danger that scientists may regard them as unwelcome kibitzers at their game. In this regard their role is similar to that of the literary or the art critic. Physicists, according to my impressions, have in recent years put up a sign: "Philosophers please keep out; reconstruction going on." Small wonder, as the printer's devil once had it: "Nuclear" physics is "unclear" physics. The majority of physicists want to unuddle themselves without the aid of philosophical clarifiers. But I have found psychologists and social scientists much more hospitable. Just like a certain small minority of physicists, they seem to appreciate a few hints from the philosophers of science. But there is on the one hand the danger of running in open doors, i.e., of laboring the obvious and the trivial; and on the other hand there is the danger of stepping on sensitive toes—especially with as large and diversified an audience as the present one. I shall have to take these risks and hope for the best. The only reassurance I can offer is that I have been concerned with the philosophy of the empirical sciences for a good many years and that, though my original scientific background was that of physics, chemistry, and astronomy, I have had also a long and enduring interest in psychological theory. This dates back at least to the year 1932 when I taught at the University of Iowa. I still remember the day when, with fear and trembling, I approached Dean Carl E. Seashore, that pioneer of American experimental psychology, and asked him whether he would approve of my offering a seminar in *Philosophical Problems of Psychology*. Seashore was

known for the forceful manner in which he discouraged projects that appeared to him as "out of line," "utopian"—or in short, as "so much moonshine." But to my great and pleasant surprise, he slapped my shoulder and said "Feigl, that's good!" Well, I have felt encouraged ever since, and in this paper I shall try to review some of the philosophical perplexities and embarrassments of psychology.

Let me say immediately that the embarrassments are mutual, they go both ways. Important philosophical issues keep bothering the psychologists; and many significant developments in psychology have baffled the philosophers. It is true that the same holds also for mathematics, physics, biology, and the social sciences; but the philosophical issues have a very special poignancy in connection with psychology.

Philosophy and psychology, only some 40 or 50 years ago, appeared so intimately related that these two fields were even administratively conjoined in one department in many colleges and universities. But with the development of the experimental techniques in psychology, there came a long series of separations and divorces. The sort of emancipations which had occurred much earlier in all the other fields of science had finally been achieved also by psychology. But, now that the divorce is fully accepted by both parties, and remarriage is clearly out of the question, there are signs that a good friendship may be mutually profitable. This seems all the more possible and hopeful because philosophy itself has thoroughly mended its ways. In fact there are many who speak of a "revolution" in philosophy. (If you should be sensitive to this "subversive" term, I shall call it a "thorough transformation"—avoiding the equally subversive label "radical change of outlook.")

Not without justification has the enterprise of philosophy been regarded with distrust and suspicion by tough-minded scientists. Speculative philosophers throughout the ages have wrestled with the riddles of the universe and have quarreled and argued in a manner that seems to exclude any responsible settlement. There are ways of stating

<sup>1</sup> Invited Address delivered at the sixty-sixth Annual Convention of the American Psychological Association, Washington, D. C., August 31, 1958.

and handling problems that make them 100% guaranteed insoluble. I shall illustrate later on how speculative thinking, usually quite unwittingly, can produce completely unanswerable questions. For the present I want to indicate, only in a general way, that the much greater sophistication of recent philosophical analysis has furnished us with an adequate diagnosis, as well as with "hygienic" and "therapeutic" measures for the prevention and the extermination, of this sort of philosophical malady. (Some colleagues in my profession still resent my defining philosophy as the disease of which it should be the cure.)

More seriously speaking, there is now a fairly generally accepted similarity between philosophical analysis and psychotherapy. Philosophical analysis attempts to make fully explicit the *conceptual* conflicts that underlie the intellectual symptoms of inconsistency or paradox, just as psychotherapy attempts to produce full awareness of the *emotional* conflicts that underlie the behavioral symptoms or maladjustments. I suspect that most philosophical quandaries are ultimately rooted in emotional tensions. I have known professional philosophers who have been so deeply disturbed by their puzzles that both philosophical analysis as well as psychoanalysis might be "interminable" with them. But, fortunately, in many cases the philosophical treatment suffices even though it is much closer to the surface. Some philosophical troubles are the result of simple ambiguities. Let me illustrate this by a perplexity that seems to have exercised lately a few prominent psychologists: the famous issue of free will vs. scientific determinism. I shall not tarry long over this hoary puzzle, but even a brief discussion might persuade you of the cathartic and clarifying power of philosophical analysis.

The perplexity of this ancient issue consists in the apparent logical incompatibility between two beliefs, each of which appears plausible on its own grounds: The assumption of free choice seems borne out by the testimony of introspection; also it seems indispensable as a presupposition for moral responsibility. On the other hand, a great deal of biological and psychological evidence points in the direction of a fairly strict determinism in regard to human behavior. "Nature" and "nurture" (i.e., hereditary constitution and all environmental influences up to the moment of choice or action) are assumed to go a long way toward determining our decisions as well as our conduct. But, so it seems

to many thinkers, if we are to be free, we cannot be enmeshed in a strict network of causal relations. Hence, the relief and jubilation in many quarters when the "good tidings" of indeterminacy in basic physics were proclaimed.

But a little critical reflection shows readily that this sort of "absolute chance," far from constituting free choice, would be experienced as a queer kind of compulsion, and thus not serve at all as a basis for moral responsibility (i.e., praisability or blamability). Only if, to a significant extent, we are the choosers of our choices, and the doers of our deeds, can we be held accountable. The entire bafflement is due to a confusion which can be easily dispelled. We must not confuse freedom with indeterminacy (i.e., the absence of causality), and we must not confuse causal determination with compulsion, coercion, or constraint. As already Spinoza essentially saw it, we are free to the degree that our choices and our conduct are determined by our character and personality. The fact that our personality and character in turn may have been completely determined by antecedent conditions does not militate against regarding our actions as a consequence of what we are at the moment of action. Apples are produced by apple trees, even if apple trees themselves are the products of seeds, soil, air, rain, and sunshine. To be unfree means no more than to be under some sort of constraint. To be free means that the chooser or agent is an essential link in the chain of causal events and that no extraneous compulsion—be it physical, biological, or psychological—forces him to act in a direction incompatible with his basic desires or intentions.

It has been asked how one can be held responsible for actions springing from a personality whose structure might have been determined by the initial and boundary conditions long before the infant was born. The answer to this question can be found by a logical analysis of the concept of responsibility. We are responsible to the extent that our behavior is responsive to the usual sanctions of society. Rewards and punishments, encouragements and discouragements of any sort can be successfully applied *only* under the conditions of at least a high degree of causal determinism. The nonresponsibility of the psychotic as well as the responsibility of the normal person can be understood only within the causal scheme of events. (The borderline cases of neurotic compulsion may

be intriguing, but have to be dealt with pragmatically and in the light of the nature of the special situation on hand.) Empiricist philosophers have long ago explicated causal necessity in terms of lawfulness and predictability.

Free will, properly understood, "presupposes causal determination (in this sense) and is inconceivable without it" (Hobart, 1934). If human acts to any extent, and through some amplification processes, were a result of basic quantum indeterminacies (i.e., on the microlevel of atomic interactions), then to that extent we could not be held responsible for our conduct. The phrase "I could have acted differently," as used in common parlance, does not imply indeterminacy of this or any other sort. It merely means "If I had been wiser or if I had had different attitudes, I would have acted differently." Under the influence of positive and negative reinforcements (some of which may well be internal to our personalities) we learn how to adjust ourselves for future exigencies and contingencies. The sentiments of regret or remorse can be instrumental in this respect only if they do not hopelessly and exclusively fix the individual's attention upon his past deeds, but rather if they are *prospective* and thus mobilize the resolution to act differently on future similar occasions. Docility, the capacity of modifying both one's beliefs and one's attitudes under the influence of cumulative experience, is of the very essence of freedom.

I hope this brief sketch will have shown that an examination of the meaning of the terms we use can go a long way toward the clarification of deeply puzzling issues. Proper attention to the two questions "What do you mean?" and "How do you know?" quite generally constitutes the most effective strategy in modern philosophical analysis. (It should be noted, however, that this is nothing really new in philosophy. Ever since Socrates conversed with the bright young men of Athens, inquiries into meaning and validity have been an important part of the philosophical endeavor; but this search for clarity has often been overshadowed, if not suppressed, by the extravagancies of speculation concerning absolute truth, reality, or values.)

Let me conclude this introductory discussion by a brief formulation of the aims and procedures of current philosophy of science. As we understand our endeavors nowadays, they are not directed toward a speculative anticipation of future scientific systems or unifying syntheses and integrations.

The new emphasis is placed on the quest for a clear understanding of the basic concepts, assumptions, and methods of the sciences. Two philosophical techniques stand out in this enterprise: One is the informal type of logical clarification through linguistic analysis. (My discussion of the free will perplexity may be taken as an example of this technique.) The other approach is that of a more formal (sometimes also quite formalistic, not to say formidable) reconstruction of scientific theories in terms of syntactical, semantical, and pragmatic metalanguages. As I see it, these two techniques supplement each other: a higher level of aspiration in regard to logical exactitude requires the first technique to be consolidated by the second one. In what follows I shall, for ease of exposition, restrict myself to the relatively informal type of discourse of the first technique.

I propose now to discuss some of the philosophical embarrassments that attach to fundamental issues of psychology as a science. Since it is not feasible to separate neatly questions of *subject matter* and questions of *method*, I shall treat both of them as they become relevant.

#### PSYCHOLOGY AMONG THE SCIENCES

It is customary to assign to psychology a place in the system of the sciences, somewhere straddling the fence between the natural and the social sciences. This is to say that, if we first distinguish the formal sciences (i.e., pure logic and mathematics) from the empirical or factual sciences, psychology is clearly one among the latter; and in some of its divisions like psychophysics and psychophysiology, as well as in the behavioristic studies of the learning processes, psychology shares many of the characteristics of the natural sciences; it is in this respect closely akin to, and allied with, biology. But when it comes to clinical psychology, psychoanalysis, the psychology of thought, to social and anthropological psychology, the relation to the social and cultural sciences appears much closer. Possibly under the influence of certain philosophical doctrines, some psychologists (but they are a minority) still insist on a fundamental difference not only in subject matter, but also and especially in *method*, between the natural and the social sciences.

Thus it has been claimed that the natural sciences are "nomothetic" while the social sciences are

"idiographic." This means that the natural sciences are supposed to concentrate on the discovery of general laws whereas the social sciences are said to concentrate on unique and individual cases and events. This seems plausible enough if one compares, e.g., theoretical physics with history. But there are idiographic natural sciences—such as descriptive astronomy, physical geography—and even such historical disciplines as paleontology. And there are clearly nomothetic social sciences such as sociology or economics. Even if you question their success in the discovery of reliable laws in these fields, the intent of the research is undeniably nomothetic. I think many of the recurrent disputes in regard to this issue can be settled by a recognition and proper allocation of the nomothetic (generalizing) and the idiographic (individualizing) components in the natural as well as the social sciences. Even if we feel that we do not have very precise and reliable theories of personality in current psychology, the very concepts which have been introduced in this discipline lend themselves to both: the description of the individual case, and the formulation of general regularities. These regularities may be only of a *statistical* character, but this still does not diminish their difference from individual description.

Cutting a little more deeply, there is the related alleged difference between *explaining* and *understanding*. Various schools of thought—mostly of German descent—have emphasized this as a fundamental distinction between the natural and the social sciences. To come down to specific examples, we are told that the regularities, e.g., concerning negative afterimages or concerning thresholds and limens in psychophysics, are not in any way "understandable" in the manner in which we can understand that frustration engenders aggression, that success leads to greater self-confidence, or that some given premises lead a thinker to draw a special conclusion. The role of the phenomenological approach, the importance of *empathy*, has been stressed by certain representatives of clinical psychology. It had of course also been an important tenet of Gestalt psychology. "Intuition," "insight," "understanding," and "empathy" have been key words in the strife of psychological movements. These terms are used honorifically by one party, but they are suspect (if not on the *index verborum prohibitorum*) with the other party.

But I think the dust and the heat of the disputes

now are clearing away. We recognize that, especially in the psychology of human motivation, and in psychodynamics generally; empathy is an often helpful and important heuristic tool. But we realize also that empathetic judgments can go woefully wrong, no matter how strong their intuitive conviction. Empathy may be a *source* of knowledge in that it *suggests* hypotheses. But it is not self-authenticating. Objective tests alone can confirm the correctness of these "hunches." The philosophical embarrassment here arises out of a confusion between the origin and the justification of knowledge-claims. Once we distinguish between the psychological roots and the methodological validation of our judgments, there remains no fundamental difference in the type of justification legitimately applied in the natural and the social sciences. This may be recognized quickly if "understanding" is seen to rest on familiarization. Familiarity breeds intuition; but it is neither a necessary nor a sufficient condition for scientific explanation. In the more advanced physical sciences highly abstract theories possess great explanatory power, but the postulates of those theories are not in the least self-evident or intuitively convincing. They are effective premises for the sort of derivation which constitute scientific explanations. We should not feel constrained to explain the new, the surprising, or the unfamiliar exclusively on the basis of old, customary, and familiar premises. To be sure, it is pleasant if it can be done this way, but, I repeat, this is neither necessary nor sufficient for a good scientific explanation.

The phrase "clinical experience has shown . . ." is sometimes intended to disparage or discourage independent objective tests. Freud, for example, despite his respect for the scientific method and his general naturalistic outlook, was not interested in the experimental confirmation of his doctrines. But though it is admittedly difficult to test psychoanalytic hypotheses, it seems imperative at least to formulate them in such a way that an experimental confirmation or disconfirmation becomes possible. I have no particular partisan axe to grind in regard to psychoanalytic theory or any other school of thought in contemporary psychology. The philosopher of science can afford, and should sincerely attempt, to remain above the battle. After all, we philosophers operate mostly by hindsight anyway. Do not expect us to blaze many new scientific



trails—those glorious times are over. Altogether too many philosophers have sold their birthright for a pot of message! (The “prophet motive” operates here! This is part of *our* embarrassment.) In view of the enormous complexity of modern scientific research, we have had to lower our level of aspiration. We are glad if we can provide a clearer understanding of the scientific enterprise. But here, I am confident, we can fulfill a useful auxiliary role; especially in collaboration with productive scientists we can serve as critics and catalysts. This is the sort of thing we have been doing at the Minnesota Center for Philosophy of Science—in connection with psychological as well as with physical theory.

After this modest advertisement I return to the issues of Understanding vs. Explaining: Hard-headed psychologists, especially those of radically behaviorist convictions, usually repudiate psychoanalytic interpretations as so much fantasy or metaphysics. But let us see whether the wheat cannot be separated from the chaff; or, if you permit a horrible shift in metaphors, whether we cannot save the baby while throwing out only the dirty bath water. As is well known, Freud himself considered his theory as “monolithic”; i.e., he did not permit his disciples to pick and choose among its postulates. Only wholesale acceptance or wholesale rejection seemed legitimate to the grand old man. But this is surely not in accordance with Freud’s own procedure. On numerous occasions he revised parts of his doctrine—no doubt on the basis of clinical experience! Even from a logical point of view it must be admitted that scientific theories consist of relatively independently testable hypotheses. It is true, and has been stressed ever since Pierre Duhem and Henri Poincaré, that there are no *strictly* crucial experiments. But this is simply a logical triviality, viz., that a particular experimental finding can never establish the truth or the falsity of a single hypothesis which is part of a whole set of postulates. But careful experimental or statistical designs can group and regroup the to-be-tested hypotheses in such a way that the probabilities of confirmation or of disconfirmation become so significant as to be practically sufficient for the (always tentative) acceptance or rejection of a given hypothesis. As we empiricist philosophers must never forget to repeat: Scientific truth can be held only “until further notice,” such notice to be given when contrary evidence crops up.

Now there are theories (and some of the Freudian doctrines are examples) which are so conceived as to be irrefutable by *any* sort of evidence. But these are the excesses of the psychoanalytic creed. When we consider the Eros-Thanatos doctrine, i.e., the attempts of explaining behavior in terms of life and death instincts, we find that it explains too much, namely, *any* conceivable type of behavior. This, far from being a virtue, vitiates the theory. Theories of this sort—I am tempted to call them manichaeian—are (usually unwittingly) made immune against any conceivable test. They are proof against disproof. They constitute a *methodological* embarrassment. The history of scientific and philosophic thought offers many parallel examples. Doctrines of material substance, of an indestructible soul, of absolute space and time, the phlogiston hypothesis in early chemistry, the ether hypothesis in its comatose last ditch stand, the speculations about vital forces or entelechies in biology, etc. have all been defended in a manner that precludes even the most indirect or incomplete confirmations or disconfirmations. Closer logical analysis reveals that the factual content claimed for such doctrines and hypotheses simply does not exist. This may come about in the following ways: Some of the key terms of the doctrine may fail to have empirical significance in that they are completely severed from any and all observation bases. This in turn may be the result either of (implicit or explicit) stipulation, or it may come to pass by a process most aptly described as “whittling away” whatever empirical meaning those key terms may have had to begin with. In the case of Freud’s manichaeian doctrine, very much like in the case of the older formulations of the pleasure-displeasure principle in British psychology, the factual emptiness results from a slide into tautology. It is like the well known rule for weather prediction: “The weather will either remain the same or it will change.” The quest for certainty, the craving for infallibility, has produced the embarrassments of *emptiness* and *circularity*. If behavior is to be explained or be predicted on the basis of Eros or Thanatos, and if the only criterion supplied for deciding which of the two to adduce is the actual conduct, then there can be no explanation or prediction at all.

It would however be grossly unfair to psychoanalytic theory if, in the light of these criticisms, we were to repudiate it altogether. There are much more substantial contributions in the theories con-



cerning the unconscious. The mechanisms of repression and of defense, the interpretation of dreams, the psychopathology of everyday life, the explanations of neurotic behavior, etc. are in principle capable of empirical examination. But here again, tough-minded operationists and radical behaviorists have criticized the theories as metaphysical and have attempted to reduce their tenable core to empirical regularities concerning the positive or negative reinforcement of behavior.

Before I turn to a more general discussion about the nature of, and the need for, theories in psychology, there are a few remarks to be made about the notion of *evidence* in psychological explanation, interpretation, and prediction. Let us take as examples the psychoanalytic view of dreams and of free associations. As I have already mentioned, the intuitive conviction carried by some of these interpretations may by far outrun anything that a scientifically minded psychologist would grant in terms of objective probabilities. The high level of aspiration in experimental science seems to prevent acknowledgment of the cognitive significance of such weak indicators as are at the disposal of the clinical psychologist or psychoanalyst.

But if we reject these indicators as evidence, we would in all consistency have to reject likewise the clues in historical research or in detective work. Straightforward experimental procedures and even explicit statistical designs are here scarcely applicable. This is a matter of stark practical impossibility. But historical and detective methods may be quite scientific in that they consist in a careful scrutiny and critical comparison of the various items of evidence. If the historian ascribes a given painting to an individual artist of the fifteenth century, he utilizes clues of various sorts and of course holds his interpretation open to further confirmation, as well as possibly to disconfirmation, by whatever relevant evidence may be discovered later on. This is not different in principle from such reconstructions of past events as we find in geology or paleontology. To be sure, if we define the scientific character of an hypothesis in terms of a criterion of strict *predictability*, most of what goes on in historical studies of any kind would be relegated to the limbo of the unscientific or the nonscientific endeavors. But surely, this is too narrow a view of the scientific enterprise. The specific and detailed features of an island that arose out of volcanic activities, or of a canyon that was formed by

erosion, could not have been predicted before these events happened. The particular shape of the devastation or the shambles produced by an earthquake are unforeseeable. The *particular* structures and functions of plants and animals as they came about in the evolution of the species will very likely remain unexplained. But the *ex post facto* explanation of the *gross* features of the respective results (i.e., the general features of the island, of the canyon, of the devastation, of the organisms) is perfectly legitimate.

*Mutatis mutandis* this may be applied to human behavior. The particular manner in which a murder or a suicide was executed would generally not have been predictable on the basis of even a fairly complete knowledge of the antecedent circumstances. Similarly, just which specific images occur in the manifest content of a dream may forever remain unpredictable. But this need not prevent a psychoanalytic interpretation of the manifest image as a symbol. Just what specific free associations will be produced in response to a certain stimulus situation may well remain equally unforeseeable. But this again does not exclude an explanation of the emitted verbal responses on the basis of assumed unconscious mental states as long as these are accessible through other independent avenues of confirmation.

The diffidence with which predictions and explanations of this type are treated by tough-minded scientists may have much to do with a fixation on the well known *deductive* model of inference. But once it is realized that the deductive model fits only some very special cases, and is applicable even to these only by way of considerable idealization, a more liberal attitude may well be accepted. There are perfectly good explanations not only in common life but also in the sciences in which the derivation of the facts or regularities to be explained from the laws and the conditions which provide the explanation is no more than *probabilistic*. Deductive necessity can be had only *more geometrico*. The classical theories of mechanics, thermodynamics, and electromagnetics were deductive systems, owing mainly to the deterministic form in which their postulates were cast. With the intrusion of probabilistic and statistical concepts even in basic physics, the deductive model must now be regarded as an ideal limit case. And in the light of pertinent scientific evidence it is highly precarious to hope for successive approximations which will take us

anywhere near this limit. (Perhaps I should remark here that my previous definition of free will does not presuppose a *strict* determinism. A statistical type of causality is perfectly sufficient for the ascription of responsibility.)

The ways and means of behavior, I conclude, may not ever be more accurately predictable than probabilistically. But this is sufficient for an (equally probabilistic) type of explanation. Out of the thousands of images or words that may occur in a given situation one only actually occurs. This specific item has therefore only an extremely low probability. But in the light of psychoanalytic theory (i.e., if we could only fully spell it out) there is always a *class* of responses which is greatly more probable than the class of all alternative responses. The difficult job to be done consists in specifying the first class in terms of its defining characteristics. Here, too, we should not and need not aspire to too much precision. Classes with vague borderlines are used commonly and profitably in many sciences. Perhaps one way of getting at the defining characteristics would be by utilizing the intuitive experiences of empathizers as a first guide. After all, something very similar is done in the construction of personality and other psychological tests. Later validations can correct whatever errors are due to the original "bootstraps" procedure. *Prima facie* this might seem to re-establish the difference between the social and the natural sciences which I have been at pains to deny. But remembering the helpful distinction between the context of discovery and the context of justification we can recognize that criteria which have their origin in clinical intuition may later be validated objectively and scientifically. We may even be able subsequently to explain the workings of intuition.

Let me illustrate this by a story (no doubt a canard) which appeared in the newspapers a long time ago. A doctor had a dog that would bark in a peculiar way when confronted with patients who had an incipient cancer. The cancers were in such an early stage of development that they could not be detected by the usual (x-ray, etc.) methods. Just how the dog managed this marvelous feat was completely obscure. But, if one wanted to be "scientific" about it, the relative frequency of "correct" barkings could be ascertained statistically. Once it is established that this frequency is significantly above chance, then the question of how to explain

this fact may be tackled. (This is, after all, the attitude many psychologists take in regard to graphology, or in regard to extrasensory perception.) I suggest that the clinical psychologist, and the empathizer generally, is his own diagnosis dog. If and only if a high "batting average" can be established, on the basis of subsequent objective evidence, is he entitled to some confidence in his diagnoses. This way of looking at the matter should satisfy even a rigorous behaviorist.

#### DEFINITION OF SUBJECT MATTER

This brings me to what is perhaps the most painful philosophical embarrassment of psychology: the definition of its very subject matter. As the well known saying goes: psychology first lost its soul, later its consciousness, and seems now in danger of losing its mind altogether. I know very well that the specification of psychology's subject matter is highly controversial. The disputes concerning it seem to be emotionally highly charged, if, for no other reason, then certainly because of the perennially puzzling mind-body problem. This problem is currently not fashionable, not even in pure philosophy. I think that the behaviorist revolution in psychology has much to do with this state of affairs. According to my own diagnosis, the mind-body problem has been repressed, and all sorts of defense mechanism have been developed to evade this issue which traditionally has been so closely tied up with theology, religion, and metaphysics. Powerful emotions are elicited whenever we touch questions of *Weltanschauung*. I do not flatter myself that I could desensitize many of you in the course of a few brief remarks. But perhaps I can at least suggest to you that repression of the problem merely produces troublesome symptoms—paradoxes and perplexities in this case—and that philosophical analysis can provide a helpful catharsis. Judging by recent publications in theoretical and methodological matters, the issues of introspective vs. behavioristic, of phenomenological vs. neurophysiological psychology crop up again and again, and with remarkable tenacity. The *Zeitgeist* may not be favorable for a revival of these traditional issues, but since I believe there is a genuine possibility for a constructive solution, I shall take the liberty of briefly sketching it out for you. (I must ask you to forgive the somewhat dogmatic tone of my presentation. It took me over a hundred pages

in an essay (Feigl, 1958) published earlier this year to deal with the major facets of this badly tangled problem.)

If we extricate the mind-body problem from its traditional theological and metaphysical background, there remain nevertheless a number of questions which are apt to produce intellectual discomfort, if not anxiety. The central issue is this: Can we give a logically consistent and scientifically acceptable account of the relations between subjective immediate experience on the one hand and behavioral or neurophysiological processes on the other? The traditional answers are well known to you. (Psychology textbooks used to mention them at least in the introductory chapter.) They are: dualistic interactionism, psychophysiological parallelism (usually in the form of epiphenomenalism), the double aspect doctrine, and the theory of emergent evolution. The endless disputes between these different attempts at a solution of our "riddle of the universe" have been so discouraging that many thinkers—in the camps of philosophy as well as psychology—have tried to undercut the whole issue. They have tried to show that this is a pseudoproblem which need not trouble us. But, as I hope to show you, these are maneuvers of evasion. Prominent among them are phenomenalism (or neutral monism) and logical behaviorism. Characteristically, the positivist movement in more than a century of vigorous influence upon epistemology and philosophy of science has embraced sometimes the one and at other times the other of these points of view. But both phenomenalism and radical behaviorism must be accused of committing reductive fallacies. The phenomenologists reduce our concepts concerning physical objects to logical constructions out of the data of direct experience. And the logical behaviorists reduce "mentalistic" concepts (including those that refer to direct experience) to logical constructions out of the publicly observable peripheral behavior. (If we are not to spend hours on formal technicalities, I shall have to ask you to let me get by with these rather loose characterizations.) It appeared for a while that we must choose between a reductive view (of either sort) and a metaphysical solution of the mind-body problem. Metaphysical solutions are abhorred by the tough-minded scientists—though they have been tempting to the speculative philosopher. I have called them "seductive" fallacies, because they arise out of wishful thinking.

Is there no *via media* between the philosophies of the "nothing but" and the philosophy of the "something more"? My own way of thinking in these matters was dubbed by my students as "Feigelian dialectics." That is to say that I discern and recommend a constructive *synthesis* of the *thesis* of tender-minded and the *antithesis* of tough-minded thinking. (You will remember that William James, a long time ago, proposed this sort of resolution of philosophical issues. James himself, however, never found a satisfactory equilibrium. He vacillated between radical empiricism and an early form of functionalism. Perhaps I should also remark that my approach has precious little in common with either Hegelian or Marxian dialectics. I do not plead for a new logic. I use the dialectical pattern only as a means of exposition.) In short, I suggest that instead of committing ourselves to either a philosophy of the "nothing but" or of the "something more," we adopt a philosophy of the "*what's what*!" That is to say that we need not impoverish the world by absurd reductions, nor enrich it by unconfirmable projections. We may, instead, follow the lead of the scientific method: ascertaining what there is and attempting to explain and formulate it in a clear, coherent, and parsimonious manner.

More specifically, I plead for a monistic, naturalistic view of the mental and the physical, allowing for a qualified reinstatement of the introspective and phenomenological approaches; and I look to neurophysiological theory for an eventual emendation and supplementation of behavior theories. At first this may sound to you either cheaply eclectic or hopelessly programmatic, if not utopian. But there are increasing indications that such a synthesis is not only logically possible, but that it is a fruitful direction of research. To bring this closer home to you, and to make it more concrete, I would say (even at the risk of shocking part of my audience) that an approach such as Donald Hebb's *Organization of Behavior* may well supplement B. F. Skinner's *Behavior of Organisms*. (I have been wondering whether Hebb had chosen the title of his book in contrast and opposition to that of Skinner's.) Clark Hull's neurophysiological speculations, it is true, were rather idle wheels in the machinery of his system, but his basic methodological orientation at least provided a place for neurophysiological theory. Edward C. Tolman's "intervening variables" may yet turn out to be hy-

pothetical constructs or theoretical concepts which will correspond to neurophysiological concepts in a more complete theory à la Hebb, perhaps in the not-too-distant future. Some of Kurt Lewin's field theoretical and dynamic concepts have been absorbed in Tolman's approach, and would in any case in principle be open to the same neurophysiological specification. This is, of course, in keeping with the original isomorphism doctrine, especially in the form given to it by Wolfgang Köhler. Add to this the promising contributions of Norbert Wiener, Warren McCulloch, and others, along the lines of cybernetics, and we have at least the beginnings of a theory of those intriguing teleological (i.e., goal directed and self-regulating) processes which in earlier times drove some philosophers, and even some psychologists, into a defeatist vitalism.

I realize that there are a great many empirical questions yet to be settled in this highly controversial area. But it does seem to me, at least from the vantage point of my philosophical armchair, that the doctrine of isomorphism has a good deal of experimental evidence in its favor. I think, moreover, that the doctrine has been subjected to unjustifiable criticisms by behaviorists and operationists. The reintroduction of introspection, the new concern with the phenomenal field, the clinical attention to subjective experience, the studies in social perception, etc. seem to me to indicate, not indeed a regression to an obsolete psychology, but rather an advance along the spiral (or should I say "helix"?) of the evolution of the scientific outlook.

The limitations of the introspective and phenomenological approach are realized and admitted. We are aware of the sources of error in introspective description and phenomenological-clinical observation and interpretation; and being aware, we can apply measures to correct these errors. If we refrain from fallaciously reducing subjective experience to response patterns, we may well reinstate the classical distinction between mental states and their behavioral manifestations. It is precisely because we have been cautioned by the behaviorists that we can do this with greater sophistication, and hence with a better intellectual conscience. And thinking the situation through to its logical conclusion, we may go beyond a timid psychophysiological parallelism and hold an identity theory of the mental and the physical. That is to say that the data of subjective experience are identical

with certain central states of behavior theory, and thus ultimately with certain neurophysiological states. Let me indicate the logic of these identifications by way of a succinct outline.

As I diagnose one of the main motives of classical behaviorism, it was the problem of our knowledge of other minds which constituted the major philosophical embarrassment. Professional philosophers have carried on agonizing discussions over this puzzle. The admitted impossibility for one person to inspect another person's mental contents seemed to conflict with the firm beliefs of commonsense. Even if, in ordinary life or in clinical observation, we claim to apprehend fairly directly or intuitively what goes on in the other person, it was rightly maintained that this—by itself—does not constitute a justification for our assertions about other minds. The obvious gambit of using analogy arguments for such a justification seemed excluded because of a fundamental difference between this particular application of analogical reasoning from the ordinary type of analogical argument. In most ordinary cases the conclusion can be independently verified with the same directness and to the same degree of certainty as the premises of the argument. But just this is fundamentally impossible in the case of the inference to other minds. Aided and abetted by the positivists, the behaviorists insisted on a criterion of meaning according to which only objectively verifiable assertions are scientifically meaningful. Introspection as such did not seem to qualify—unless interpreted (as already suggested by John B. Watson) as a response to previous responses. The objectively certifiable behavior of organisms, along with, and in the same sense as, the processes and events in the lifeless or inorganic realm, was considered the only legitimate subject matter of science. No wonder then that behaviorism (as Max Mayer put it so strikingly in the title of his 1921 book) became "the psychology of the other one." (To be sure, there are other facets of behaviorism, such as the emphasis on the experimental method and the faith in environmentalism; but these are irrelevant in the present context.)

You may feel that since the battle of behaviorism was fought some 30 or 40 years ago, that there is no point in reviewing it today. But I surmise we might get a very different picture in a current opinion poll among psychologists. Even the positivists have mended their ways. They have liberalized their erstwhile extremely restrictive meaning cri-



terion. That is the main reason why most of them prefer to label themselves as "logical empiricists" or "scientific empiricists." These new party designations are intended to convey the shift from verifiability to *confirmability*. That is to say that we no longer insist on complete and direct testing, but that we consider any statement as scientifically meaningful if it is at least incompletely and/or indirectly testable.

The problem of other persons' mental states thus appears in a very different light. As long as we conceive of mental states, be they our own or those of others, as part and parcel of a network of lawfully related facts, they are no longer inaccessible to confirmation, i.e., to indirect verification. The phenomenal data of one's own experience are of course susceptible to direct verification, they are open to immediate inspection. And in keeping with our commonsense convictions, we have every reason to identify these directly inspected or inspectable states with mental states imputed to us by others or, after a little scientific refinement, with the central states of modern behavior theories. What will *not* work in this connection is a purely peripheralistic behaviorism. As I have pleaded before, peripheral stimulus situations and response patterns may serve as (probabilistic) indicators of central (i.e., mental) states, but they cannot be identified with them. Even B. F. Skinner—perhaps the most consistent among the behaviorists—now has a place for something like private central states in his conceptual scheme of things. Of course this privacy is not the "absolute" privacy of the metaphysicians. It is the relative, practical privacy familiar from everyday life and admitted by any psychologist who accepts confirmability, but does not insist on direct verifiability.

The embarrassment of unanswerable questions can be avoided if we do not introduce absolutely unconfirmable entities into our theories. Such puzzles as that of the "inverted spectrum" illustrate this vividly. This ancient poser has been revived a great many times and caused innumerable philosophical headaches. The question is: Even if two subjects A and B perform equally well in thorough color discrimination tests, could they not "privately" experience different color qualities? Could there not be a systematic difference between A and B, such that A experiences, e.g., green when B experiences red, and vice versa, throughout the whole circle of pure hues? If it is

further assumed that all other behavior tests, and even neurophysiological investigations do not reveal any significant individual differences between A and B, then the scientifically minded psychologist feels he can rest his case and assert that the color experiences are similar in A and B. But the metaphysician insists that there might be differences in the private experiences which remain forever and absolutely incapable of objective test.

Shall we just shrug our shoulders and give up on philosophers who make life so difficult for themselves? Perhaps we can attempt a little philosophical therapy. We might show the philosopher that he has illegitimately extended ordinary or empirical doubt and thus fallen into metaphysical doubt. Ordinary empirical doubt may occasionally be hard to settle. But if it is transformed into metaphysical doubt, then there is no conceivable way of settling it by either logical argument or by empirical demonstration. Will this cure the metaphysician? Not necessarily. He may grant that unanswerable questions should be kept out of science, but that they are a worthy subject of puzzlement for the philosopher. But if the philosopher remains forever puzzled, this is a most frustrating state of affairs even to him. Perhaps we can suggest that the philosopher's mistake consists in conceiving of color experience merely on the basis of his direct acquaintance with the qualities he experiences, that he lifts this concept artificially and arbitrarily out of the rich network of causal relations which constitutes the meaning of the concept in ordinary discourse, and even more fully so in scientific knowledge. Perhaps we can show him that on the basis of all these lawful relations his concepts of mental qualities can be recognized as denoting the very same referents that terms of behavior theory (and ultimately of physiological theory) also denote.

This view of co-reference of different terms, as I have already hinted, may well be the key to the solution of the mind-body problem. And it would thus also solve, or rather *dissolve*, the embarrassing puzzles concerning our knowledge of other minds. As long as, in the spirit of the scientific method, we require that it must in principle be possible to *give reasons* for our knowledge claims, we must never entertain knowledge claims that are entirely and absolutely divorced from, and hence unsupported by, empirical evidence. Philosophical doubt concerning other minds is in this respect similar to philosophical doubt about the occur-



rence of past events. There is no logical guarantee—so the skeptic argues—that the laws of nature might not have changed from period to period, and that therefore our inferences of past events are hopelessly precarious. He might even go so far as to say that there was no past at all and that the universe originated with a bang a second ago, with all the so-called remnants (geological strata, fossils, memories, etc.). This is just as irrefutable as is solipsism, but of course just as insanely irrational. A little reflection suffices to show that the very meaning and essence of rational belief, as we understand it in common life and in science, includes most prominently the “normal” procedures of inductive and analogical inference, as well as of the hypothetico-deductive or hypothetico-probabilistic methods of theory construction.

The mind-body problem need no longer be evaded or repressed. Advances in science as well as in logical analysis have helped in freeing us from its metaphysical shudders. An explicit uncovering of the various functions of language and of the corresponding types of significance may break whatever resistance still remains against the sort of identity or co-reference view I am advocating. Language serves all sorts of purposes. It may be used for the sake of the formulation and the communication of information. This is *cognitive* meaning. But it may also express or evoke images, emotions; and it may be used in eliciting action or in the molding of dispositions or attitudes. The alleged gulf between the qualities of the mental and the physical, I submit, are largely due to the images which we connect with the use of *physical* concepts. “How can the processes in central nervous systems or the cortex be identical with mind or subjective experience?” If you conceive the brain as that grayish mass you perceive upon opening a skull, or if you visualize the protein molecules of the nervous tissues in terms of the tinkertoy models chemistry instructors show to their classes, then it is indeed impossible to form any consistent idea of mind-body identity. But this is picture-thinking. Pictures and illustrative models are a help, heuristically or didactically. But even in basic theoretical physics such pictures are now at best regarded as an intellectual scaffolding which is to be removed once the building of knowledge has been erected. “Thou shalt not make graven images unto thee!” The proper factual content of physical theories is strictly speaking unvisualizable. It

consists in the abstract conceptual formulation of the postulates which are connected by correspondence rules (usually probabilistic ones) with the observables in various domains of evidence. The description of a neural process in abstract physical terms may therefore be interpreted as referring to the very same events which may be described also in phenomenal language on the basis of introspection.

Psychological theories, such as those of molar behaviorism or those of Freudian psychodynamics, use theoretical concept which, perhaps after some revisions or refinements, may well dovetail with certain concepts of neurophysiology. I can see no objection in principle or from a *logical* point of view against the possibility of such a final identification. Any serious doubts about it must be empirical ones and concern the explicability of behavior on a neurophysiological basis. In this connection both the psychologist and the philosopher had better be cautious and await further evidence. For example, the facts of parapsychology—if indeed they are facts and not due to experimental error, hoax, or fraud—might well force upon us a thorough reconsideration of the very framework of presuppositions in psychophysiology. But there is, as far as I know, not even a glimmer as yet of an acceptable and responsible theory of these curious phenomena. As an empiricist I have at least to go through the motions of an open mind on this topic.

#### PROBLEMS OF THEORY CONSTRUCTION

Let me conclude this long paper with a few more explicit remarks about the problems of theory construction in psychology. The embarrassment here is a *methodological* one; and to the historian and logician of science it is familiar from parallels in the development of physics and chemistry. There have been proponents as well as opponents of theory construction both in the physical as well as in the behavioral sciences. The philosophical perplexity consists in the question: What do theories enable the scientist to do that he could not do with the aid of straightforward empirical generalizations? The two parties to this dispute are the theoreticians and the radical empiricists (we call them “dustbowl empiricists” at Minnesota). I must leave it to you to diagnose and to explain the temperamental differences between these character types; this is of course a matter for psycho-

logical or cultural-anthropological study. I will only suggest that radical empiricism has a good deal to do with the wish for intellectual security, i.e., with the desire to restrict one's extrapolations to the domain in which they have been thoroughly tested; it also has a good deal to do with the fear of the invisible and intangible. Hypothesis-phobia has often been a personality trait of positivists—that is why they are such *negativists*! The theoreticians on the other hand do not mind living dangerously. This seems commendable as long as they keep their theories open to revision. Fixation on theories is of course a frequent weakness. But rivalry, competition, and fierce criticism are the order of the day and help in the avoidance of dogmatism.

But enough of this dilettante psychology. I wish to tackle the question from a *logical* angle. This requires a little reflection upon the aims of science. If we distinguish the applied sciences, with their aims of control and guidance, from the pure or fundamental sciences, then the aim of the latter may be defined as the attainment of adequate descriptions and explanations. Prediction, vitally important in all practical applications of science, enters pure science primarily as a means of checking the adequacy of laws and theoretical assumptions. It should not be necessary to defend the distinction between description and explanation. Explanation always involves inference or derivation, deductive or probabilistic, as the case may be. Among the premises of an explanation we need at least one law-like assumption, deterministic or statistical in character. It is expedient to restrict the meaning of the word "description" to singular statements of specific facts—or to conjunctions or disjunctions thereof. This makes for a clean distinction of explanation and description.

Now, according to the radical empiricist, the cognitive, factual content of science consists in the descriptive facts and in the regularities formulated in empirical laws. Empirical laws are usually conceived as fairly directly testable functional relations (again, either deterministic or statistical) between observables. It is my impression that the radical empiricists confuse "factual content" with "cash value." The term "cash value" is not intended here in a pejorative sense. I use it merely to designate the kind of information which is eminently useful in the practical applications of scientific knowledge. I would argue, however, that in

the very interest of obtaining this sort of "cash value," *theories* have become extremely helpful, and in many fields indispensable. But quite apart from the practical applications, theories have the distinct value of helping us to *understand* the world and man's place in it. Man does not live by bigger and better technologies alone! Through a sort of "heterogony of purposes" (Wundt) or through "functional autonomy" (Allport) civilized man has acquired curiosities which can be satisfied only by adequate and comprehensive explanations. Such explanations can be achieved only with the aid of theories. A good theory enables us to *derive* empirical laws—and not only those which we have already attained by simple generalization, but also others which still remain to be certified experimentally. Theories are thus not merely convenient and compendious summaries of empirical laws, they are heuristically fruitful in that they enable us to infer further, as yet untested regularities.

But right here our antitheorists ask: Suppose we had investigated all empirical regularities of a given domain, would a theory then be more than, at best, an equivalent though compressed and possibly elegant formulation of a set of empirical laws? My answer is in the negative—and for the following reasons. First of all, most of the so-called empirical laws of science are not as empirical as they appear to be. The concepts in terms of which they are formulated are rarely open to straightforward operational definition. I am referring here not only to the usual idealizations and simplifications experimental scientists apply in the formulation of empirical laws. I have in mind especially the *ceteris paribus* clauses which are often implicitly taken for granted. This is tantamount to guessing at a functional relation between certain key variables—a functional relation which would manifest itself in measurements and experiments, if it were not camouflaged by certain obfuscating variables which are not sufficiently known and hence largely beyond control. The empirical laws that psychologists confidently incorporate in their textbooks are thus not as directly testable as they are often made out to be.

Operationism, just like behaviorism or other forms of radical positivism, has had an undeniably helpful influence in the development of recent science. Untestable assumptions have been relegated to the limbo of metaphysics. But it is time

to recognize that radical operationism would curtail the enterprise of science very severely indeed. Of course, "operationism" is now often understood in the much more liberal sense of a methodological attitude, insisting on clarity in regard to the rules—according to which we use our terms. And if these rules concern the observational-experimental *as well as* the logico-mathematical procedures, then "operationism" simply becomes synonymous with "good scientific method" and loses its distinctive reductionist emphasis. But the earlier more restrictive operationist ideology is not at all in agreement with the actual procedures of science.

I have anticipated the essential point here already in some of my earlier remarks. Radical operationists, very much like the logical positivists of some 20 or 30 years ago, are so fixated on direct verifiability that they never analyze the implications of (indirect and incomplete) confirmability. But just this is the essential feature of all sciences which advance beyond the observational, fact-collecting ("botanizing") stage. The radical operationist maxim "different operations, different concepts" is severely at variance with the actual manner in which the meaning of theoretical concepts is specified. The most important syntheses and unifications in science depend on the introduction of theoretical concepts through convergent avenues of specification, coming from different, often at first very heterogeneous areas of evidence.

I admit that for a while it did seem plausible to view many scientific concepts as dispositional and hence to define them by means of test operation→test result conditionals. For example, habit strength in Hullian psychology, or expectancy in Tolman's, seemed dispositional in the sense that, though direct observation is impossible (just as in the case of magnetic strength in physics), these concepts could be regarded as shorthand for certain stimulus-response or (more generally) certain empirical regularities in the sequence of test operations and test results. This procedure was formalized by R. Carnap (1936-37) in a very influential essay more than 20 years ago by the device of reduction sentences. But Carnap (1958; see also Hempel, 1958) himself has abandoned this approach, mainly for the reason that even open sets of such reduction sentences do not adequately reconstruct the actual role of theoretical concepts and their relations to the observables.

Ever since the incisive article by MacCorquodale

and Meehl (1948) we at Minnesota, as well as Carnap, have found it helpful to distinguish between purely empirical dispositional concepts ("intervening variables" in the narrower sense) and hypothetical constructs (or "theoretical concepts," as they had better be called). Theoretical concepts are introduced by postulates which, as it were, define these concepts implicitly. The meaning of theoretical concepts is thus given by their place in the network of laws—the "nomological net" as we label it. The nomological net is what Einstein used to call a matter of relatively "free construction." It is anchored only in a few places in the facts of observation. Or, in less picturesque language, theories are postulate systems which derive their empirical meaning and factual reference through the correspondence rules by which the theoretical concepts are connected with certain concepts in the observation language. This allows for the empirical confirmation (or disconfirmation) of theories; it also provides for the multiple routes from various domains of evidence to the theoretical concepts; and it provides for a legitimate way in which to include statements about unobservable entities in our theories.

Since most correspondence rules are not simply explicit definitions or semantic designation rules, but rather probabilistic indicator relations, it is clear that the cognitive content of theories cannot be translated into observation statements, not even an infinite set of these. Modern physics certainly cannot be interpreted in any other way. The concepts of the atomic and the quantum theories refer to unobservables, but they are related to observables in many various areas of evidence. It seems to me that the situation in psychological theory is quite analogous. Just as in physics, we may begin with the formulation of empirical laws. But empirical laws almost always are severely limited in their range of validity. For more comprehensive integrations we have to ascend to higher levels of explanations. This usually results in corrections to be applied to the empirical laws with which we started. Molar behavior theory—and psychodynamics may be construed as part of it—is in many respects methodologically analogous to classical ("phenomenological") thermodynamics. There is clearly a good measure of explanatory power present in theories of this type. But just as physics had to rise to the next higher level of explanation by introducing the molecular theory of matter and the

kinetic theory of heat, so molar behavior theory is now in the initial stages of being reduced to neurophysiology.

I realize that many psychologists feel threatened by this sort of reduction. They think that their own subject matter of research is being supplanted by another one in which they do not feel at home and which they regard as beyond the limits of psychology proper. As a philosopher of science I can afford to view these developments with equanimity. Anyone (like myself) interested in the adventure of the unification of the sciences will even welcome these interdisciplinary transgressions. But the experimental and the clinical psychologists may in any case rest assured that a tremendous amount of work remains to be done on their particular level of research. I would merely plead that neurophysiological theory may yield important fruit much sooner than many molar behaviorists seem to believe.

Space does not permit me to deal with other philosophical perplexities of psychology. There are many other embarrassments, and one can never be sure to have selected the ones that are most poignant to one's readers. The whole area of value judgments and its relations to scientific knowledge is in urgent need of thorough clarification. There are also a number of topics much closer to the issues I did discuss, and which I had to omit from this paper.

By way of an *envoi* let me say that modern

psychological research and modern logical and methodological analysis may have much to offer to each other. We have made a promising start in the collaboration between creative science and critical, clarifying methodology. The time has come to emancipate ourselves from the radical empiricism of the operationists and the behaviorists. A more liberal view of the nature of scientific theory will help us more adequately and clearly to assign to psychology its proper place in the uniting sciences and to remove many of the philosophical embarrassments that have stood in the way of scientific progress.

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## AN OPINION ON THE CONSTRUCTION OF BEHAVIOR THEORY

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ON both intellectual and pragmatic grounds, present day theory in the behavioral sciences is far from satisfactory. At few points does behavior theory hold the intellectual excitement or the genuine aesthetic appeal that is present in concepts of the older sciences. All too often, it seems little more than a stuffy reformulation of common sense principles which gains some precision at the expense of clumsiness and pedantry. From a practical standpoint, behavior theory has failed to suggest important new lines of research and has proven comparatively sterile in application to the vital problems of society.

Of course it is hardly fair to contrast the present status of theory in the behavioral sciences with that of the physical sciences which have a much longer history and which even now command a far greater portion of societal effort. At the same time, the achievements of the physical sciences stand before us as an example of human ability to extract meaning and order from the baffling and occasionally fearsome aspect of nature. Although it would be most unrealistic to expect the imminent appearance of such a drama in the behavioral sciences, it is not out of order to inquire whether we are even setting the proper stage for it at present.

Full justice to the topic here suggested would require an extensive critique of the contemporary climate of theory development as well as a constructive strategy for behavioral science theory in general. The more limited purpose of this paper is to propose a set of five focal points about which a fuller examination of the problem might be organized.

### *Big Theory*

For some years the growth of behavior theory has been marked by an increasing concern with

details and by the burgeoning popularity of the "miniature system." Fundamental reappraisal of the major systems of psychological thought has given way to improvement and exploitation of selected features of these systems. To borrow a phrase of Whitehead's, modern behavior theory is becoming a series of footnotes to the original theoretical systems, and footnotes in ever smaller type.

Several historical factors may be responsible for this narrowing of theoretical scope. First, there has been a tendency for the dominant schools of thought to adopt a "with us or against us" attitude which does not encourage searching examination from within. Since the accepted pursuit of a theorist within an established framework is exegesis, any attack on core concepts must come from other schools. Such external criticism may still be superficial simply because the critic does not have a sympathetic understanding of the concepts he is assailing. A rather different influence in the fractionation of theory has been the very rapid advance in the technical facilities available for laboratory and field experimentation. Gadgets, both physical and conceptual, have accelerated the discovery of behavioral phenomena which were in no way envisaged by previous theory. Such findings clamor for some measure of theoretical benediction and the resulting formulations are inevitably circumscribed.

Of course it is possible that miniature theory itself, by successive expansion and annexation, may lead back again to general theoretical frameworks, but this process contains certain hazards. Although the crucial experiment is a rarity or a myth in the behavioral sciences, miniature theories do encounter embarrassing results. They tend to adjust to these by the growth of strange appendages which may effect their temporary survival but which make them difficult to assimilate into a community of ideas. Further, while miniature theory is well adapted to subtle and penetrating attacks on specific problems, it is totally blind to the "end runs" that might be indicated if the problems were seen

<sup>1</sup> This paper was completed while the author was on leave of absence as a National Science Foundation Senior Postdoctoral Fellow at Harvard University.



in larger perspective. Thus it is possible to devote inordinate time and effort to topics that could be treated more naturally in a different order of priority. In fact, even in the solution of specific problems a strict miniature theory approach does not capitalize fully on isomorphisms and analogies with other areas.

The need for continual generation and revision of major theoretical doctrines, however, goes well beyond the foregoing, largely tactical, considerations. The time must be anticipated when the behavioral sciences will again have to face the perennial questions that were more or less swept under the rug as an aspect of our emancipation from philosophy. No amount of experimental virtuosity or model-building ingenuity seems sufficient to cope with the problems of belief, attention, consciousness, and experiential quality. A comprehensive and integrated view of human behavior, however, will surely require servicable answers to the questions suggested by these topics. The mere fact that Big Theory has had mostly foolish things to say about them in the past is grounds for caution but not for despair.

#### *Immunity from data*

Properly developed, the process of abstraction permits the inclusion of constantly wider realms of empirical phenomena within the purview of a few, relatively simple laws. The reverse side of the coin is that the implications of abstract schemata become increasingly, perhaps exponentially, difficult to unravel as abstraction progresses. This suggests that a patient and thorough investigation of the internal dynamics of a conceptual schema is required before the schema is confronted with the brute facts of the empirical world.

In the behavioral sciences there is a strong tendency to expect the data to do our thinking for us. The typical reaction to a new theoretical suggestion is for friends and foes alike to start marshaling experimental testimony—this in spite of the widely acknowledged point that very few theories are finally accepted or rejected by sheer weight of evidence. Aside from its otiosity, however, this reaction has an unfortunate influence on the form of a developing theory. The insistence that theory should be immediately descriptive and experimentally fruitful tends to atrophy its long range growth as an abstract system. In conse-

quence, the behavioral sciences presently contain very few abstractions—only empirical generalizations and afterthoughts to those generalizations.

There are several strong historical factors that may account for this condition. The two epoch-making scientific events of the century have both been interpreted as triumphs of the operational approach—broadly, the analysis of constructs in terms of the experimental conditions essential for their measurement. There is a natural reluctance to abrogate a principle which lists relativity theory and quantum mechanics among its successes, yet any departure from first-order descriptive theory seems to require just that. As against this powerful argument it may be remarked that these two theoretical “breakthroughs” are the product of immense sophistication as well as of operationism. It may well be that the solid bricks of operational theory can be laid only after a scaffolding of less durable materials has been erected—as it had in the physical sciences.

A second historical factor that has led to impatience with the inner life of behavior theory is the strong applied emphasis within the psychological profession. Either by choice or by necessity a large proportion of behavioral scientists are engaged in educational, clinical, military, and commercial practice that demands a shirt-sleeve demonstration of the validity of theoretical assumptions. For this purpose, the more direct and uncomplicated the applicability of a principle, the more impressive it is and the less vulnerable to the dreaded “longhair” label.

It has often been suggested facetiously that good theories have been spoiled by the data—this is here maintained as a serious, though qualified, proposition. Ultimately, theory must answer to the facts, but this is not the only requirement placed on a theoretical system. Logical consistency, economy of assumptions, and even a degree of elegance are by no means secondary factors in determining the overall staying power of a theory. These patrician qualities are quite unlikely to mature, however, if the demand for direct descriptive capability is too insistent. It is not then contended that theory construction should be totally irresponsible to the general body of knowledge about behavior. Rather it is held that point-by-point testing of isolated facets of a theory against specific behavioral phenomena or experimental findings is at odds with the whole purpose

of theoretical abstraction. Suggestive hypotheses should not be put directly to drudgery but should be entertained for a while, as rare and welcome guests.

We need genuinely speculative theorizing; yet, if any psychologist today had notions as bizarre as those involved in current nuclear physics, he would suppress them or visit a therapist (who would perhaps employ the exotic notions of Freud). Sooner or later, however, we shall require constructs that offend common sense, and it is entirely possible that the deduction of common sense or experimental consequences from those constructs will require more time and more brain power than any single individual can muster.

This proposal therefore has a firmer motivation than the one of making life more tranquil for the ivory tower theorist. It is intended to prevent an excess of theoretical "errors of the second kind," i.e., total rejection of hypotheses which may have some substrate of merit. The history of science contains many examples of principles that were discredited at one stage or another because they did not square with current observations: the heliocentric hypothesis, the hypothesis of corpuscular light propagation, the environmental modification of genetic traits, and the spontaneous generation of life. In each case, these principles have later been revised in a somewhat altered form in the face of the same old facts. Such cases argue strongly in favor of a stay of execution for theoretical principles against which there is only circumstantial evidence.

#### *Scientific License*

Any model or theory represents an abridged picture of the real world and this abridgement may take various forms. In the physical sciences we can distinguish three major types of validity. First, a theory may be valid in a *subjunctive* sense. That is, it may describe the behavior of entities under conditions that never obtain in fact. The Galilean law of falling bodies and the ideal gas laws are examples. Second, a theory may be *locally* true, that is, may hold over certain ranges of the relevant variables. Hooke's law of elasticity is a clear cut example, and the Newtonian laws of motion (holding for "middle-sized" phenomena) are now accepted in this sense. Finally, a law may hold *statistically*, being supported by large numbers of

observations although there are local exceptions. The standard example is the law of increasing entropy. In each case, the postulated "law" can be defined as the potential limit of observational approximations as certain stipulations are satisfied.

It may well be that behavioral laws must invoke all three of these forms of license in relating predictions to the world of observation. In other words, we may be reduced to predicting relationships among parameters of statistical distributions which can be empirically sampled within certain limits only, and which are subject to extraneous disturbing factors. It may further develop that completely new ways are needed of putting theoretical statements into correspondence with observed data, or of describing their failure to accord with the data. Certainly one can imagine limiting processes and simplifying assumptions other than those employed in the physical sciences. A vigorous study of these might greatly facilitate the progress of substantive theory.

A few brief examples may clarify what is intended by the suggestion. First, it is quite customary to cushion hypotheses concerning behavior with a generous *ceteris paribus*. But there is no *paribus* and it is often apparent that even the form of a functional relationship must be quite contingent on unspecified variables. What is needed here, in very broad terms, is a means for defining the boundaries of psychological and social systems and for characterizing the exchanges that take place at those boundaries. Again, the notion of rational behavior is an example of a fiction that has been highly useful, or at least highly used, in economics. Psychologists feel rather uncomfortable with this concept, but they do not have to accept Hobson's choice. Surely there are degrees and dimensions of rationality (or of altruism, etc.) that can be described and can serve as parameters in the formulation of behavioral laws. As a final illustration, our present state of uneasy truce with teleology might be turned into a real alliance. It should be possible, that is, to use to conceptual advantage the powerful and obvious phenomena of organism adaptation, without falling prey to the error that everything is for the best. Functionalism went far in this direction but never attained a completely acceptable solution.

The critical point here is that, if behavior theorists knew how to be more explicit about their violation of the facts, they could then afford to be

rather more self-indulgent. The widespread over-concern with theoretical verisimilitude has been discussed in the preceding section. A consequence of this conservatism is that we end up by importing capital goods, such as information theory and game theory, when indigenous products might be better adapted to our needs.

The failure to be explicit, on the other hand, imposes a sort of double uncertainty on the interpretation of behavior science theory. No one expects behavior theory to apply precisely to the real world but, as things stand, it is often hard to know just what allowances can and should be made. The familiar caveats with which theoretical papers and research reports are punctuated have somewhat the force of a cryptic "poison!" label on a bottle. The warning might better indicate under what conditions it is toxic and what some of the antidotes may be.

### *An Inspired Semiotic*

Many of the great forward surges in scientific thought may be traced to what appear at first blush to be merely ink-saving devices. The calculus notation of Leibnitz, the structural symbolism of organic chemistry, and the index conventions of tensor analysis are particularly striking examples. Many of the laws of modern physics would almost certainly not have been obtained through ordinary verbal reasoning, and in fact defy translation into unambiguous everyday language. It seems unlikely that the behavioral sciences will produce abstract theory worthy of the name until they have developed conceptual tools of comparable power and economy.

The need for a suitable language for behavioral science has of course been recognized and there have been various attempts to meet it. On the one hand there have been studies of the etymology and usage of familiar terms, searching for acceptable kernels of meaning. Pulling in the other direction, special groups and schools have adopted terms that may sharply describe their own subject matter but are incapable of transfer to other fields. From the standpoint of long range theoretical needs, both of these approaches appear to be seriously misguided.

The principle limitation of everyday language for theoretical purposes arises not from its semantic fuzziness—although that is serious enough—but

from its syntactic awkwardness. Behavior theory, in particular, is not concerned with the precise description of special types of things. Instead, it attempts to define the relationships among certain broad classes of events: environmental events, response events, and neural events. The very possibility of achieving a unified body of theory depends on the indifference or fungibility of most qualitative properties of these events. Any general laws of behavior, in other words, must be invariantive with respect to all but gross attributes of their subject matter. This seems dangerously close to being a truism but much of the vast effort of factor analysis, for example, appears to deny its import.

Refinement of definitions does not guarantee that the words will become more useful as conceptual tools. In fact, the investment required seems to increase the temptation to use the final product to explain phenomena and cut off further analysis. What is required is a symbolism that makes perspicuous the known relationships among referents and that suggests the nature of new relationships. The handicaps in using ordinary language for this purpose are very great. Words are designed to be put in sentence frames, and the customary sentence (even a sentence of Proust or Faulkner) is not well adapted to expressing complex scientific propositions. Of course it is not suggested that behavioral scientists should immediately abandon their native tongue, any more than physicists or organic chemists have done so. However, the gradual development and adoption of an auxiliary symbolic notation seems an absolute prerequisite for any precise articulation of behavioral laws.

It may be useful to summarize in a more positive, if general, way the specifications for such an auxiliary language. What seems indicated is a set of common denominator terms or "primitives" and a definite convention governing the embroidery of these symbols for special purposes. This elaboration, or compounding of terms, might take the form of indices or coupling operators of some kind. As much as possible, both the atomic terms and the rules for compounding them should be as free of doctrinaire bias as the familiar mass, length, and time units of physical theory. Optimistically, this would permit individuals to hew to their unique substantive concerns and to air their idiosyncratic theoretical bents without in any way sacrificing the unity of the science at large.

### Model Mathematics

There is good reason to suppose that the mathematical forms in which behavior theory ultimately finds adequate expression will be quite different from the mathematics of the physical sciences. First, the basic data of behavior simply do not conform to the ground rules for the highly developed number systems on which most physical theories are based. Much more fundamental, the operations to which these number systems are adapted—i.e., addition, multiplication, etc.—are of remote relevance to the interplay of organism behavior. Finally, the overall problem of the behavioral sciences seems different: it is not to account for precise relationships among a few variables, but to elucidate low order regularities in extremely complex variable systems.

The disparity between extant—or at least accessible—mathematical tools and the requirements of our data has led to a rather unfortunate situation. Certain areas of behavior theory have been subjected to an almost absurd degree of mathematizing, while other phenomena have in effect been out of bounds. There have indeed been attempts to extend the range of phenomena under mathematical dominion as well as to broaden the mathematical repertoire in use. However, in most cases these have been isolated *tours de force* or else they have involved the adaptation of existing mathematical systems, usually with some damage to the latter.

From the outset, the physical sciences have enjoyed a close interaction with mathematics and most of the great physicists have been at least competent mathematicians. Often their treatment of problems has been heuristic and results have undergone a great deal of later scrutiny and revision by more fastidious mathematicians. But they have provided a challenge for mathematicians and have traced out vast regions for mathematical exploration. In this function behavioral scientists seem somewhat remiss, even granting the difficulty of their subject matter. There are certainly very few examples of significant mathematical investigations stimulated by the problems of behavioral science.

It is possible to point to mathematical topics of great potential relevance to behavior theory that have not yet had a real trial in this capacity. As one example, only a few very special types of integral equations have yet been investigated thor-

oughly, and there have been few published biological applications except for population studies. The potential applicability of integral equations to learning theory, however, and to other phenomena in which the influence of the past is of central interest, is quite evident. Classical probability theory, as another example, is largely based on the concept of independent events, a concept that is utterly unrealistic for most social phenomena. Any hope for a "statistical mechanics" of social behavior may rest on the development of a probability calculus that places contingencies among events and conditional probabilities in the center of its focus. Both of these topics are extremely difficult and this accounts in part for their slow development. Another reason, however, is the fact that the problems for which they might ideally be the techniques of choice can be handled (less adequately) by other methods. Hence no very great pressures have even built up toward their more complete exploitation.

Instead of tempering problems to available mathematical techniques, or eschewing mathematics altogether, it would be extremely useful if behavioral scientists made an effort to formulate their problems and theories in such a way that the major points of mathematical intractability were clearly shown. In some cases, only a translation from the vast mathematical arcana may be required, while other problems may suggest totally new mathematical forms. Of course mathematics will not be produced to order, but a certain amount of selective fertilization should be a much more likely process than the present chances for parthenogenesis.

### CONCLUSION

Five aspects of theory construction have been selected as requiring increased emphasis in the behavioral sciences:

1. General theory on a par with miniature systems development
2. Interim protection of theory from empirical results that are in *prima facie* opposition
3. A more sophisticated and more explicit determination of the sense in which theoretical assertions are applicable or valid in the real world
4. A working language for theoretical expression based on simple ideographic terms and lending itself to symbolic manipulation



5. A truly reciprocal interaction between substantive behavior theory on the one hand, and mathematics and logic on the other

These features are all present in the comparatively successful natural sciences. They have appeared only in isolated or fragmentary form in the behavioral sciences and have never really caught hold. Often the translation of these characteristics of the physical sciences has been too literal and has been handicapped by unrealistic expectations. The current tendency is to react to the dismal record of theory in the behavioral sciences by relegating it to an even more subordinate role. At present, theory is widely supposed to be a sort of file clerk in the great warehouse of certified observational facts.

The present suggestions are based on the conviction that theory can and must assume as integral a position in the behavioral sciences as it has in physics. To fulfill this position it must be capable of extending our ideas and perspectives beyond the limits of existing experimental data and verbally mediated intuition. Metamorphically, theory is a means for charting a variety of possible worlds that include the observed world as an interesting subcontinent. For this purpose it must have a good measure of autonomy and must be outfitted with suitable gear. The suggestions offered in this paper represent a preliminary attempt to identify the directions of liberalization, and the types of equipment that seem most clearly essential.



# THE PSYCHOLOGY OF THE RECESSION<sup>1</sup>

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THE most pessimistic business forecast which I have seen is contained in a confidential memorandum prepared by a well-known business economist in the spring of 1958. During the past 15 years the business outlook was governed by sustained high confidence, the economist says. Then he continues: "The kind of confidence experienced prior to the second half of 1957 probably will not be a characteristic of a period consisting of a number of years, possibly a decade, which began toward the end of 1957."

Is this prediction trustworthy? This is a question of great importance for the future of the American economy because, and I quote the expression used recently by the First National City Bank of New York, one of the country's largest banks, in its March 1958 *Monthly Letter*: "demand depends on income and confidence." Traditional economic analysis, operating with such variables as income, sales, assets, debts, and prices, does not suffice to answer the question about the future course of the economy. Government experts, business leaders, and economists need information on the state of confidence and the factors influencing the fluctuations of consumer and business sentiment.

## NEED FOR ECONOMIC PSYCHOLOGY

In order to provide this information economic psychology has been developed during the past 20 years. Economic psychology has two basic propositions. The first is the one I have just quoted from the bank report and is fairly old. More precisely, it states that demand is a function both of ability to buy—that is, of income, assets, debts, etc.—and of willingness to buy—that is, of changes in optimism or pessimism, confidence or uncertainty, feelings of security or insecurity. The second proposition of economic psychology is of more recent origin: Changes in willingness to buy

are measurable. Changes in consumers' attitudes and expectations can be measured through repeated interview surveys conducted with representative samples of the population. Such measurements contribute to an understanding of the fluctuations in one of the most volatile areas of economic behavior, the fluctuations in consumer expenditures on durable goods.

From World War II to the end of 1957 the American economy was prosperous, even though the advances in standard of living were occasionally interrupted for a short while. In 1958, we experienced the first real downturn in the economy since 1937. From the fall of 1957 to the spring of 1958 the rate of industrial production fell from 145 to 126, and unemployment rose from 2.5 to 5.2 million. During the past few years many people thought that depressions belonged to the past; recently it became clear that the instability of our economy is still one of our most pressing problems.

Traditional economic theory has postulated that fluctuations in the business cycle originate in the business sector of the economy—in increases or decreases of business investment—or occasionally in government deficits or surpluses. It was assumed that only business and government can generate income; consumers were relegated to a minor role under the assumption that they spent the income they received from business and government at a steady rate. Yet over the past few decades there have been some major changes in the structure of the American economy. Today in the United States a broad middle-income group spends a substantial portion of national income on discretionary or postponable purchases and even supplements its income through borrowing or drawing on accumulated reserves. The course of the American economy may now be influenced by millions of decision makers, consumers, who may accelerate or retard the rate at which they purchase houses, automobiles, and other durable goods.<sup>2</sup>

<sup>1</sup> Invited Address delivered at the Annual Convention of the American Psychological Association, Washington, D. C., September 1, 1958 at the invitation of APA Divisions 9 and 14.

<sup>2</sup> Both in size and in rate of fluctuation, consumer investment in houses, automobiles, and household durables

In traditional business cycle theory, psychological factors were disregarded or made responsible for small and short-lived deviations from the regular functioning of the market place which was thought to be governed primarily by monetary factors. This kind of analysis may have been justified in societies in which consumers had spent practically all their incomes on necessities and a small number of entrepreneurs behaved like economic men or, if they did not do so, fell by the wayside during periodic depressions which were characterized as necessary cleansing processes. When, however, broad groups of consumers have some latitude of action, analysis of their behavior becomes a task of major importance.

Since both ability to buy and willingness to buy are essential, analysis of economic behavior must be concerned with financial interrelationships as well as with people's motives, attitudes, and expectations. Even in the first area, psychology does play a role because survey research constitutes one of the methods through which financial data, for instance on the distribution of incomes, assets, and debts, are collected. Although the Survey Research Center of the University of Michigan has been engaged for many years in the collection of data on financial and demographic interrelations, in this paper I shall restrict myself to a discussion of psychological factors. But I may mention that the same surveys which give us information about the changes in people's incomes, bank deposits, debts, and purchases, as well as about their age, education, and family composition, are used to collect data on optimistic or pessimistic attitudes, on feelings of confidence and uncertainty, on hopes and fears (Katona, 1951).

#### PSYCHOLOGICAL ANALYSIS OF RECENT CONSUMER BEHAVIOR

I turn to the presentation of findings. I shall describe, far too briefly, the attitudinal information collected by the Survey Research Center which is

is today as large as or larger than business investment in plants, machinery, and inventory accumulation. In 1957, consumer investment amounted to approximately \$45 billion; business investment to \$31 billion. From the first quarter of 1957 to the first quarter of 1958, consumer expenditures on durable goods fell by 13%; business capital expenditures by 11%. Other consumer and business expenditures are much less variable.

relevant for an attempt to answer the following three questions:

1. What were the major characteristics of the past 15 prosperous years?
2. How did the recession of 1958 come about?
3. What happened during the recession?

#### *Psychology of Prosperity*

Three sets of attitudes developed after World War II and most pronouncedly in the early fifties. First, optimism, feeling of confidence and of security have prevailed. People believed that they were moving ahead. Younger and middle-aged people held only two opinions regarding their economic situation in the next 5 or 10 years: namely, that income and standard of living would be higher or the same. As years go by, one would acquire more and better things of life. The income level prevailing at a given time was seen as the lowest possible.

Optimism has generated what has been called "thing-mindedness" and upgrading of possessions. Achievement of progress brings about a rise in levels of aspiration. The consumer, having obtained a raise, desires a house. Having bought a house, he is hardly saturated—rather, he needs many things for the house. Gratification of needs was found to be associated not with reduction in motive strength but with the arousal of further needs and desires. Failure and frustration, which would lower aspirations, were not experienced by masses of people.

At the same time, most Americans were convinced that a depression such as we had in the thirties was not possible. They thought that we had learned how to avert depressions and that the government would utilize this knowledge if necessary.

Optimism has been associated with the baby boom which since 1950 has taken the form of a substantial increase of three- and four-children families. It has also been associated with enormous mobility. Rural areas, small towns, and central city districts of large towns have not grown: the population increase has been concentrated in rapidly growing suburbs and outlying districts of cities.

In other words, and this is the second major characteristic of the past prosperous years, young Americans have decided that they want babies and higher standards of living at the same time. They

have decided, to be more specific, to marry early; to have several children in the early years of marriage; to bring up their children in a "nice" neighborhood (with a back yard, far from juvenile gangs); and to have a car, a washing machine, a refrigerator, a TV set, and innumerable other things.

How have millions of families accomplished all this? To some extent it seems that, not only do expenditures depend on income, but income also depends on consumer needs. Needing and wanting expensive things, people strove hard for higher incomes. Also, millions of husbands have held second jobs and millions of wives have returned to work after their children grew up. Most importantly, however, *installment buying* has helped. Buying on installment has become increasingly popular. Some older people still have misgivings about being in debt or are concerned with the cost of borrowing. But most younger people maintain that things should be paid for while they are being used. When income goes up—or even earlier, when income increases are anticipated—needs can be gratified through buying on installment and adjusting the monthly payments to the future higher income level.

The third major and sustained experience of the past 15 years was inflation. Repeated studies made by the Survey Research Center have shown that most Americans held definite attitudes toward inflation. First, inflation is bad. People resent inflation because they look at prices from the consumer point of view; they hate to pay more for food or a car than 6 or 12 months ago. Price stability and even small price declines, on the other hand, are viewed as good for the economy.

Second, people commonly believe that what goes up must come down. Even though they have not had this experience recently, and even though in answer to direct questions they often say that in a few years prices will be higher than today, they have a deep conviction that inflation has limits and the trend must be reversed sooner or later.

Third, people believe that they do not have it in their own power to safeguard themselves against inflation. Only a very small proportion of wage earners believe that their income goes up because prices rise. Wage and salary increases are viewed as rewards for accomplishments. Price increases are felt to hurt even if fully compensated by wage

increases; higher prices detract from enjoying the fruits of one's labor.

Inflation is bad, people think. Instead of responding to inflationary news by buying in advance and in excess of needs so as to beat the price increases, most of the time American consumers have reacted by restricting their postponable purchases. Thus, rather than adding fuel to the fire, they have helped to limit its ravages—but they have not helped enough to extinguish the fire.

### *The Origins of the Recession of 1958*

In order to understand the onset of the current recession we have to go back to the origins of the last great wave of prosperity. In 1953 following the truce in Korea, some cutbacks in government expenditures and in business inventories were introduced and production fell slightly. Pessimistic predictions made by some experts about the inevitability of a major depression received wide publicity in the winter of 1953–54 (Katona & Mueller, 1956). But consumer sentiment was not affected greatly, and by the summer of 1954 it improved substantially. Figure 1 shows that an Index of Consumer Attitudes prepared by the Survey Research Center advanced sharply in 1954, at a time when incomes were stable.<sup>3</sup>

Questions asked in sample surveys about the reasons for the more optimistic notions disclosed that many people were "favorably disappointed" because they, themselves, were not hurt by the much advertised recession. At the same time the price situation as perceived by consumers had a favorable impact because of widespread knowledge of high trade-in values and discounts.

The figure shows that in 1954 consumer attitudes improved several months before consumer purchases began to grow. Economic statistics show that the upswing began in the consumer sector, specifically in a large increase in consumer expenditures for automobiles and other postponable purchases. Consumer opinions that "this is a good time to buy" provided the spark to the boom in

<sup>3</sup> The index has been derived from answers to eight questions asking about expectations of being better or worse off, expectations of good or bad times, expressed buying intentions, etc. (Katona & Mueller, 1956). In Figure 1, the points of the index are placed at the time when the attitudes were determined and published. How large durable goods sales were at the same time became known several months later.

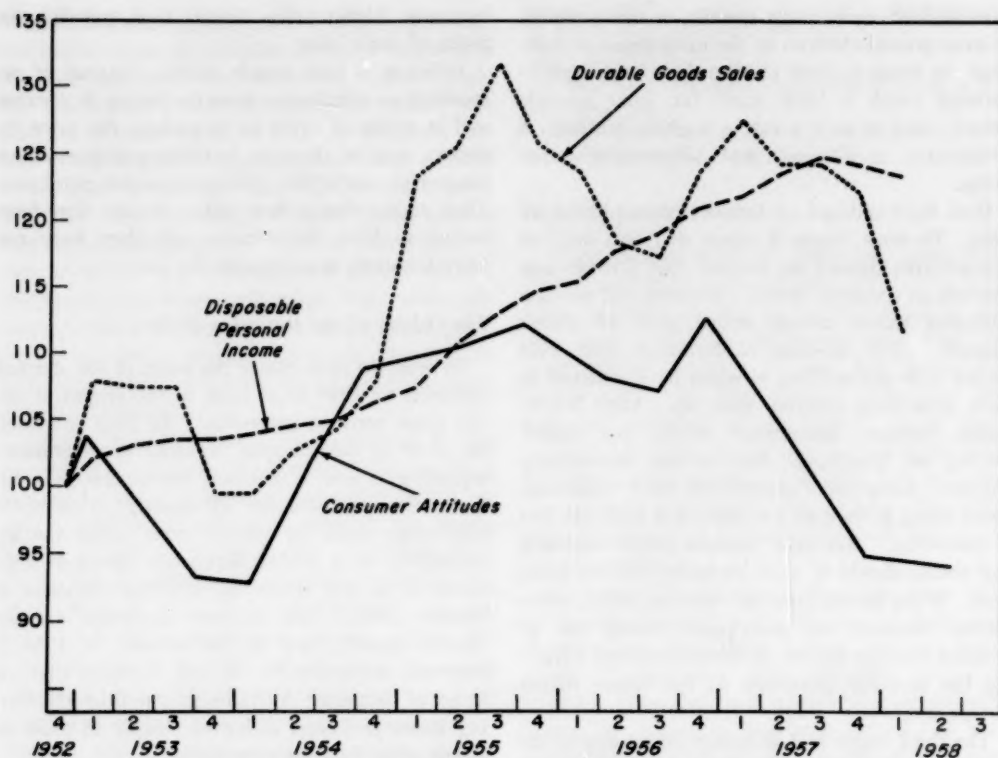


FIG. 1. Index of durable goods sales, disposable personal income, and consumer attitudes. (Sources: Durable goods sales and disposable personal income are based on United States Department of Commerce quarterly data, seasonally adjusted; last quarter 1952 = 100. Consumer attitudes are based on Survey Research Center data; Nov.-Dec. 1952 = 100.

automobile and household equipment sales. A most favorable reception of the 1955 model cars and a lengthening of installment credit maturities followed later and added to the boom.

In 1955 and 1956, consumer sentiment was governed mainly by rising incomes. Following increased sales to consumers, American industry embarked on a large-scale program of constructing new plants and re-equipping old ones. While in 1954 consumer sentiment improved when income and sales trends were stable, in the following two years measurement of attitudes did not provide indications different from traditional financial statistics. Parenthetically, it may be mentioned that attitudinal measures are useful even in such an instance; they indicate that economic forecasters may rely on extrapolations of income and sales curves.

On the basis of a survey of consumer attitudes and expectations conducted in June 1957, the Sur-

vey Research Center reported that consumer optimism was weakening (Figure 1; Foundation for Research on Human Behavior, 1957). Among the reasons for the decline in consumer confidence, absence of new encouragement was paramount. Consumers constantly need new evidence to support their optimism. It was interpreted as a sign for caution when people got the impression that business was leveling off rather than continuing its advance. At the same time concern with rising prices created dissatisfaction with market conditions and gave rise to uneasiness. Rising interest rates affected the buying of homes and also contributed to dampening optimistic views.

In the fall of 1957 there was some good news such as record personal incomes and high level of consumer spending; there was also a good deal of bad news about government and business expenditures. Surveys showed that for consumers the unfavorable news was salient. By December 1957



a substantial proportion of consumers thought that a recession had already developed. Though many of these people admitted that they, themselves, were well off, they became worried about their jobs and incomes. Concern with inflation grew, and there was a sharp increase in the proportion of people who felt that these were not opportune times for buying automobiles and other durables. The Sputniks also added to the deterioration of confidence. For most Americans, communist achievements, armament race, and international conflict have unfavorable connotations which are carried over into other areas (Foundation for Research on Human Behavior, 1958).

### *What Happened During the Recession*

The American people's ability to buy remained high during 1958. The decline in national income was insignificant because some groups still received wage increases and others unemployment compensation. On food and on necessities people spent substantially the same amounts as a year earlier. Yet there was a very sharp decline in production.

The steepness of the decline was due to the restriction of business investment and consumer investment at the same time. Business firms, after having expanded their facilities greatly in 1955 and 1956, cut their inventories as well as their expenditures for new plants and machinery when prospects appeared less favorable, and even more later when profits fell. Consumers cut their purchases of cars and other durables when they became worried about their jobs and especially when unemployment became widespread. Neither in the products offered nor in prices did consumers find any encouragement for buying cars.

Thus we recognize the operation of some self-reinforcing features: the anticipation of rising unemployment and the news that unemployment actually increased brought forth the same reaction. Doubts about the advisability of trading in one's car for a new car were reinforced when people learned that car sales had slumped, in other words, that relatively few people decided to buy new cars. By June 1958, 68% of the American people said that times were worse than a year ago; 35% that they, themselves, were worse off; and 51% that their chances for getting a new job, should they lose their current job, were bad. All these percentages were much higher than at any time

during the preceding years (Institute for Social Research, 1958).

Occasional layoffs or longer unemployment affected a substantial proportion of the population. The official statistics, according to which there were over 5 million unemployed and over 62 million employed persons during the spring and summer of 1958, present a succession of pictures which are not connected with each other. The Survey Research Center found that, while only 6% of heads of families were unemployed in June 1958, an additional 8% reported that sometime during the past 12 months they had been unemployed. Among the 52 million families in the country, about 15 million contain only people who are either self-employed or outside the labor force (housewives, retired, students). Of the remaining 37 million families, about 8 million were hit by unemployment and many more by shorter working hours.

In spite of receipt of unemployment insurance and other benefits, most unemployed reported not only that they had cut down on their purchases but also that they had done one or two of the following: decreased their savings, borrowed money, got help from relatives, or piled up bills. Yet in June 1958 the average unemployed person was only slightly more pessimistic in his economic outlook than the average person not affected by unemployment. In evaluating their chances for getting their old job back, or getting another job that would pay about the same, the unemployed expressed fairly confident opinions. Thus unemployment in 1958 was something entirely different from unemployment in the thirties.

After its sharp decline, production leveled off in the summer months of 1958. The crucial question we have to raise is whether a study of attitudes and expectations sheds any light on the probable trend. Before discussing this question, a few words about methodological and theoretical considerations basic to economic psychology are in order.

### THE FUNCTION OF ATTITUDES

Three interrelated aspects of studies of motives, opinions, and expectations in the economic area—in brief, of economic attitudes—will be discussed.

#### *Attitudes and Action*

That attitudes are predispositions to action has been widely recognized. In many areas of social

behavior, however, psychological studies remained on the level of verbal behavior. In the economic field it was possible to study the relation of attitudes, and of attitude change, to such frequent action as the purchase of automobiles, houses, and household goods.

Different reactions to similar stimuli may best be illustrated by a study of inflation. The old mechanistic theory according to which inflation is an automatic result of rising quantity of money, or of incomes rising faster than the supply of goods, has few adherents today. People's reactions to inflationary stimuli—past price increases, news about government spending, and the like—are important. Reactions to inflationary stimuli may differ greatly according to prevailing perceptions and attitudes. One possible reaction consists of trying to beat the price increases, that is, of buying in advance and in excess of needs before prices go up further. A second reaction to price increases is to reduce discretionary and postponable purchases. People may consider price increases unjustified and resent them. They may become preoccupied with how to make ends meet—how they will pay for meat and milk in view of rising prices—and postpone purchases of durable goods. Both reactions were observed in the United States in the last 20 years; but, as said before, during the last few years the second reaction prevailed.

#### *Attitudes and Prediction*

It was reported before that at certain crucial times—in 1954 and 1957, for instance—measurements of changes in attitude proved to have predictive value for later trends in consumer purchases. There exist some misconceptions about the predictive value of attitudes. It has been said, for instance, that the Survey Research Center asks questions about what people will do or what will happen to their income and that in responding to such questions people predict the future. It is true that the Survey Research Center asks questions about the future and asks questions to which even experts do not know the answers—for example, "Will there be good or bad times during the next five years?"—from people in all walks of life, from corporation managers as well as unskilled workers. But people in answering these questions express their prevailing attitudes rather than make predictions. Asking for expectations is an indirect

way of assessing current sentiment. The data obtained serve to understand what is going on at the time when the expectations are held. Good diagnosis in turn helps in making predictions. But forecasting remains a separate step, additional to and different from the measurement of prevailing motives, attitudes, expectations, and intentions.

One would not expect attitudes to have predictive value if shortly after their measurement, unexpectedly, war broke out. Even lesser events as occasionally introduced by government or business, such as substantial price increases or changes in the down-payment requirements for mortgage or installment debt, may create a new situation. This does not detract greatly from the usefulness of attitude surveys because such events are generally known and the surveys help in ascertaining their probable impact. The principal condition of using attitudes in forecasting rests on the assumption that, in the absence of unexpected events creating new conditions, attitudes and expectations will not change abruptly after their measurement. This assumption is in accord with psychological theory on the formation of attitudes. It has also been confirmed by experience with surveys of economic attitudes, provided the predictive value of attitudes was restricted to mass behavior during the subsequent six or nine months.

The predictive value of attitudes for individual behavior is of great psychological interest even though it is hardly a problem of economics, which is concerned with changes in the total number of automobiles purchased in the country rather than with the purchases of John Smith or Jim Miller. Panel studies showed (Mueller, 1957) that people who were optimistic bought somewhat more durable goods during the following six months than people who were pessimistic. Yet the correspondence between attitudes of individuals and their subsequent behavior was much less close than the correspondence between attitude trends and aggregate behavior. Attitudes of individuals fluctuate much more frequently than the average attitudes of all people or of broad income and occupational groups. Sometimes changes in the attitudes of some people cancel the changes in the attitudes of other people. Whether this finding would be repeated if we were to study deep-seated and often elusive motives and attitudes rather than "surface" attitudes is a question not yet answered. But recent research has

progressed from more situationally determined attitudes (e.g., feelings of better off or worse off) to attitudes more closely linked to personality traits (e.g., satisfaction with the standard of living) and to hidden or even repressed images and motives.

### *Social Learning*

Of crucial importance are simultaneous unidirectional changes in the attitudes of many people. Sometimes such radical changes were observed as 40% "optimistic" and 60% "pessimistic" attitudes at Time Point 1, and 60% optimistic and 40% pessimistic attitudes at a subsequent Time Point 2. When in the aggregate a substantial change in attitudes was observed, the individual changes were predominantly in one direction. The population then consisted of two major groups: those who shifted in one direction and those who did not change. This situation could be distinguished from vacillation or instability of attitudes. A positive relation was established in this situation between information received and attitude change. News and information were interpreted in the same manner by members of broad groups and were reinforced through personal contact and discussion. News conflicting with the ensuing trend was suppressed and swimming against the current rare even for people with contrary personal experiences. The process thus observed may be characterized as social learning, which was found (Katona, 1958) to have great impact on business cycles.

No doubt ultimately economic psychology may not only contribute toward the solution of socially important problems—inflation, recession—but may also be of use to psychology itself. Economic psychology is not applied psychology. Though it draws on methods, concepts, and principles of psychology, it requires theoretical concepts and empirical research of its own. The economic behavior of individuals and of masses offers unique opportunities for studying problems of habit formation, motivation, attitudes, and social facilitation and especially for studying the influence of intervening variables on overt action.

### LOOKING AHEAD

My last task is to discuss one of the urgent problems of the day: Will this be a short and mild, or a lengthy and deep, recession? In other words,

do we have reason to accept the prediction, quoted at the beginning of this paper, about a decade of unfavorable economic climate?

Described earlier were the underlying attitudes and motivational patterns which have characterized the past, prosperous years. Have these attitudes been impaired or even destroyed recently? According to studies carried out in June 1958 by the Institute for Social Research, the answer is "no."

### *No Changes in Basic Attitudes*

In repeated nationwide surveys the following question was asked: "Do you think that something like the depression of the thirties is likely to happen again during the next five years or so?" Substantially the same answers were obtained in June 1958 as during the previous prosperous years. Both times the majority of people, and about three-quarters of upper income people, said flatly that a depression like that of the thirties would not or could not happen. The proportion of fearful or distrustful people grew only insignificantly in 1958.

Turning to personal financial expectations, we find that in June 1958, just as in past good years, most families fell into one of two groups: those who expected to be better off and those who expected to be in an unchanged situation. Only 1 in 10 expected to be worse off, the same proportion as in 1953-54 and only insignificantly more than in 1955-56.

People's wishes and desires are studied by asking them to name the special or unusual expenditures which they would really *like* to make. In replying to this question about 70% of families described at least one, often several desires, while 30% failed to mention anything. This last proportion again did not increase during the current recession.

Recent findings about the American people's attitudes were reassuring also with respect to inflation. We said before that fear of inflation and unfavorable reactions to rising prices contributed to uneasiness and uncertainty in 1957. In 1958, though food prices continued to advance, many people argued that recession and inflation cannot take place at the same time. They thought that business conditions make it necessary for sellers of automobiles and durable goods to offer large discounts and "good buys." Therefore the proportion

of people expecting rising prices declined, and the proportion expecting falling prices increased.

Because the lessons learned during the past prosperous 15 years had not been forgotten and because fears of inflation had subsided, many people thought in June 1958 that we would have a relatively short and mild recession.

Recent survey findings make it appear improbable that consumer demand for durable goods will turn down further. And the findings provide no support for the prediction about the onset of a new era of lack of confidence. Yet an early upturn in consumers' discretionary expenditures is far from assured. It is conceivable that the relatively optimistic attitudes just described will be disappointing. Especially the emerging expectations about a more favorable price trend and good buying opportunities may remain unfulfilled.

#### *Consumer Discretion and Economic Stability*

Economic trends are not automatic results of operations that take place in the impersonal market place; rather they depend on the behavior of consumers, business firms, and the government. It is possible to influence consumer and business behavior and thus to help overcome a recession. Anti-recession measures may be intended to generate additional income or greater confidence. In order to be effective, anti-recession measures must serve both purposes. Appeals and moral suasion alone do not work. Likewise, additional income provided by government expenditures which create mistrust and anxiety cannot prove helpful.

One form of business action which is likely to arouse favorable response by consumers, and which many people even confidently expect, consists in lowering prices and offering good buying opportunities. Another helpful business action may consist in offering consumers a more desirable assortment or more attractive products through an accelerated rate of innovation.

It has been reported that people expect the government to act when unemployment rises, rather than to wait and see. One form of possible government action appears to arouse mixed reactions: Increased defense spending was found to have the unfavorable connotation of international conflict and the threat of war. It is seen as necessitating higher taxes rather than making wheels turn and employment grow. Public works, like building

schools and roads, are generally approved—but experts know that it takes a long time until such projects are started in sufficient number.

Tax reductions are the quickest means by which the government can demonstrate that it does act and through which both additional purchasing power and confidence in good times may be created. Even though many people were found to think that tax reductions were not possible in view of the need for large defense expenditures, overwhelmingly people were in favor of this measure. Other people do not understand how tax reductions contribute to increased demand, which finding points toward the need for consumer education. Nevertheless, much could have been achieved if taxes had been reduced early in 1958, but the damage does not appear to be severe.

The damage brought about by the recession of 1958 is not irreparable because the American consumers are overwhelmingly sane. Rather than alternating between exuberant optimism and deep despair, they think that trees do not grow to heaven when times are good and they search for good signs when most of the news is unfavorable. To be sure, extreme behavior has occurred, but only after the sanity of people has been crushed by repeated severe shocks.

We are told nowadays that consumers are puppets in the hands of unscrupulous or hidden persuaders and manipulators. It is true that consumer thinking is not fully logical, and frequent repetition influences consumer choice, especially when the choice does not matter much. But, on the whole, the broad middle-income groups have been found to have a concise and meaningful picture of the workings of our economy. They want to understand and try to understand the reasons for developments that take place.

Assertions and rumors may be accepted for a short while by some people but do not sustain action by very many people over a prolonged period unless people understand why a certain trend—be it recession or inflation—does develop. The fact that today our economic fate is dependent on millions of people, the consumers, has not added to the instability of our economy. Possibly it has made our economy even more stable. This may be the case because fluctuations of consumer demand are not shrouded in uncertainty. Changes in both ability to buy and willingness to buy can be deter-



mined, even though economic psychology has still a long way to go in analyzing the factors influencing consumer reactions. To progress further, economic psychology requires participation by more scholars—economists and sociologists, as well as psychologists—and requires greater financial support than it has obtained up to now.

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# STATUS, JOB SATISFACTION, AND FACTORS OF JOB SATISFACTION OF STATE INSTITUTION AND CLINIC PSYCHOLOGISTS

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IN this report we will compare psychologists with other mental hygiene professionals—psychologists in state schools and hospitals with psychologists in clinics. Comparisons will be in terms of status, job satisfaction, and factors of job satisfaction. Results bearing on certain hypotheses (below) will be presented. The comparisons and results bearing on the hypotheses should have implications of value to state institution and clinic administrators and personnel and to staffs involved in the education and training of psychologists. The analysis will provide perspectives in which we as psychologists can observe ourselves as professional persons.

This report is drawn from a project which surveyed five mental hygiene professions in state institutional and in nonstate institutional settings. Our ultimate purpose is the investigation of some factors associated with the problems of staffing state institutions. While the shortages of students (Albee & Dickey, 1957) necessarily limit the supply of mental hygiene workers, presumably, there are particularly objectional aspects of state institutional positions which account for the severe understaffing (National Association for Mental Health, 1954) which we assume is greater than in nonstate institutional settings. Kalinowsky (1956) reports that more favorable personnel distribution and related conditions obtained in England when state subsidy of psychiatric consultation became adequate and when changes in the manner of operation were made.

Our theoretical point of view is that commitment to a state school or hospital, despite a flourishing mental health movement and the resulting mitigation of the stigma, is still perceived by both public and patient as an act of social rejection. The mental institutions and their employees acquire low status, perhaps because of their role and association with those social deviants who are the so-called mentally ill. The relationships among mental health professions and with the public are involved

here (Zander, Cohen, & Stotland, 1957). We postulated that the aura of low status of the state schools and hospitals might deter prospective employees and that low job satisfaction associated with the state schools and hospitals would account for high rates of employee turnover. We derived the following general hypotheses:

1. Job satisfaction and status would be related directly.
2. State institutional positions would be perceived universally as having lower status.
3. The job satisfaction of state institutional personnel would be lower than that of nonstate institutional personnel.

## METHODS

The subjects in this report are 80 psychiatrists, 80 psychologists, 80 social workers, 80 teachers, and 80 nurses; each professional sample is divided into state institutional and nonstate institutional groups of 40, making 10 groups with a total of 400 professional subjects. In addition, 59 upper middle class and 51 lower middle class subjects were selected on the basis of area sampling in Columbus, Ohio. All subjects were selected from within Ohio. The majority of the subjects were selected from large urban settings. The state institutional employees were selected from six different state hospitals and schools; the nonstate institutional professionals were selected from 27 different clinics, agencies, general hospitals, public schools, and from private practice.

All institution psychologists are state employees in four mental hospitals and one school for defectives. The majority of clinic psychologists—about  $\frac{3}{4}$ —were selected from 13 state-supported clinics; about  $\frac{1}{4}$  of the clinic psychologists were selected from three VA clinics.

We used the magnetic board rating technique (Rettig, Jacobson, & Pasamanick, 1958a) for the ratings of status and for the measurement of fac-

tors of job satisfaction. The subjects filled out a 21-item questionnaire which yielded scores of objective status. Finally, to obtain measures of job satisfaction, we used a scale similar to the one used by Bullock (1954) consisting of five Likert type items with three to five alternatives. The reliabilities of the measuring techniques are treated extensively elsewhere (Rettig et al., 1958a). The nature of the interviews is described in an unpublished study.

### RESULTS

When raw measures of status are used, there is limited support for the hypothesis which states that status and job satisfaction are related. Pearson correlation coefficients between job satisfaction and objective status, and between job satisfaction and expected status (from general public, own profession, and opposite partner), generally are not significant with either group of psychologists. When expected status is compared with aspired status so that we have the discrepancy between where the subject believes he should be and where he expects to be rated, significant relationships appear (Table 1).

The second hypothesis which states that the status of state institution psychologists is lower than that of clinic psychologists is generally substantiated. All groups, including both groups of psychologists and both lay public groups making a total of 12 groups ( $N = 510$ ), accord the clinic psychologists higher status than the institution psychologists (sign test,  $p < .001$ ).

The differences in status expected from all (total) professional groups in this study are as follows: The clinic psychologists (mean 71.3, sigma 13.02) which is significantly higher than that of the institution psychologists (mean 68.1, sigma 15.01;  $t = 2.407$ ,  $p < .01$ ). Differences in status expected by clinic and institution psychologists from each individual professional group are not significant.

Generally, there are no significant differences in ideal status. An exception, and contrary to the hypothesis, is that psychiatrists in institutions accord higher ideal status to the institution psychologists (mean 74.8, sigma 12.73) than the private practice psychiatrists accord to the clinic psychologists (mean 67.8, sigma 16.50;  $t = 2.139$ ,  $p < .05$ ).

The third hypothesis which states that there are

TABLE 1

PEARSON CORRELATION COEFFICIENTS BETWEEN JOB SATISFACTION AND DISCREPANCY BETWEEN ASPIRED STATUS AND EXPECTED STATUS IN INSTITUTION PSYCHOLOGISTS AND CLINIC PSYCHOLOGISTS

	Status expected from profession	Status expected from opposite partner
Institution Psychologists ( $N = 40$ )	-.32*	-.32*
Clinic Psychologists ( $N = 40$ )	.45**	.34*

\*  $p \leq .05$

\*\*  $p \leq .01$

differences in the job satisfaction of the two groups of psychologists finds some support. The clinic psychologists (mean 3.9, sigma 1.01) believe that the people they work with are more satisfied than do the institutional psychologists (mean 3.2, sigma 1.01;  $t = 3.431$ ,  $p < .001$ ). Although clinic psychologists had higher job satisfaction means generally, the difference between the two groups on the remaining items and the total are not significant.

### Comparisons of Psychologists with other Professions<sup>1</sup>

On the average, psychologists have lower income than psychiatrists but higher income than the social workers although there is some overlap (Fig. 1). The same is true of objective status with perhaps more overlap with social workers. Groupwise, the psychologists are younger than all other professions except nurses. Psychologists overlap with the nurses in job satisfaction levels, having about the same mean, which is the lowest for the professions in this study.

Psychologists generally attach least importance to "regular work hours," "security," and "patient's respect" and more importance to "freedom in one's work" (Fig. 2). However, these factors bear little relationship to the job satisfaction of psychologists, generally (Table 2). In the subjective experience of what his job supplies, psychologists rate "regular work hours" high and the remaining

<sup>1</sup> The discussion in this section is based on group ranks, and no significance tests were made, except where reference is made to the table in which levels of significance are indicated.

factors generally low with "intellectual stimulation" and "status and prestige" being lowest for the five professions (Fig. 2). In no other group is "intellectual stimulation" so highly related to job satisfaction as in psychologists (Table 2).

#### *Comparisons of State Institutional Psychologists with Clinical Psychologists*

Compared with the other paired professional groups, there were few significant differences between institution and clinic psychologists (Fig. 3). The two groups of psychologists appeared to be more similar to each other than do the other professional pairs.

One interesting difference between the two groups of psychologists is in variability. The clinic psychologists have consistently lower variability on statement of job satisfaction items, less variability on the ranking of professions, rating of statement of job satisfaction in both importance and supply. The clinic psychologists average higher pay. The clinic psychologists are younger and have greater objective status than the institution psychologists.

With respect to the factors of job satisfaction, there is only one significant difference in importance: the clinic psychologists attach greater importance to "type of patient"<sup>2</sup> (Fig. 3). In both groups of psychologists, the pattern of correlations between the subjective experience of supply of the eight factors of job satisfaction and work satisfaction are similar (Table 2). The "type of pa-

<sup>2</sup> This is a difference that generally occurs among all mental hygiene groups, the nonstate institutional group attaching greater importance to the "type of patient" (Rettig, Jacobsen, & Pasamanick, 1957).

tient" appears to be more important to the clinic psychologist than to the institution psychologist (Table 2).

#### DISCUSSION

There is partial support for the hypothesis which states that there is a relationship between job satisfaction and status. A significant relationship is observed when status expectations are compared with an internal individually regulating criterion such as the subject's status aspirations.

The status of state institution psychologists is lower than that of clinic psychologists, when the general public is the reference group. Since state institution psychologists have lower status and job satisfaction and since status and job satisfaction are related, there is some support for the point of view expressed earlier: that is, the status of the institution apparently affects the status and job satisfaction of the employees.<sup>3</sup> One alternative explanation for the findings is that institutions may select a different person than the noninstitutional setting. With other groups, probably the psychiatrists, this may be more readily demonstrated; but  $\chi^2$  techniques showed no significant differences in such variables as number of advanced degrees, types of schools attended, experience—in fact the majority of both groups had had work experiences in the opposite setting. Apparently, then, some factors inherent in the institution adversely affect the status of its employees.

It was observed that the status expectancies of clinic psychologists are directly related to job

<sup>3</sup> While psychologists are under discussion here, these results were found with the total institutional and non-institutional samples (Rettig et al., 1957).

TABLE 2  
PEARSON CORRELATION COEFFICIENT BETWEEN *D* SCORES\* OF THE FACTORS OF JOB SATISFACTION AND  
JOB SATISFACTION FOR THE INSTITUTION AND CLINIC PSYCHOLOGISTS

	Intellectual Stimulation	Pay	Status & Prestige	Regular Hours	Security	Freedom	Patient Respect	Type of Patient	Total
Institution Psychologist	.70***	.45**	.49**	.19	.09	.46**	.05	.07	.52***
Clinic Psychologist	.62***	.12	.35*	.03	.33*	.12	.09	.25	.32*

\* This *D* score is the discrepancy between the rated supply and the rated importance of a factor of job satisfaction.

\*  $p \leq .05$

\*\*  $p \leq .01$

\*\*\*  $p \leq .001$



FIG. 1. Mean objective status, income, and age of professions. ( $N = 40$ )

satisfaction and that for the institution psychologists, the relationship was inverse. This may be a function of orientation toward different groups of reference and identification by the two groups of psychologists.

Although most of the professional groups (including the psychologists themselves) expect the "general public" to accord significantly higher

status to the clinic psychologists than to the institutional psychologists, in the groups' opinions of what that status of the two groups ought to be (ideal status) there are no differences. The psychiatrists were the group that differed: the psychiatrists in private practice accorded lower ideal status to the clinic psychologists than the state institutional psychiatrists accorded to the institutional

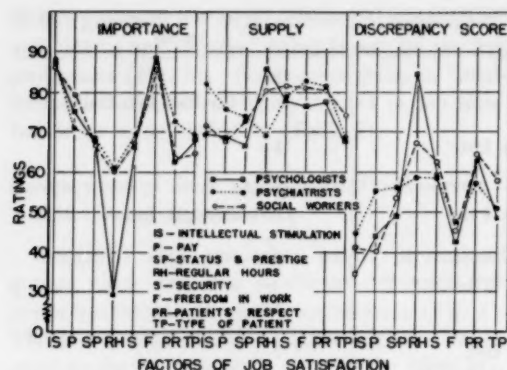


FIG. 2. Patterns of importance, supply, and discrepancy scores on the factors of job satisfaction among psychologists, psychiatrists, and social workers.

psychologists. Patently there are different working relations between psychologists and psychiatrists in state institutions on the one hand and between psychologists in clinics and psychiatrists in private practice on the other. Ostensibly, at least, institutional personnel are engaged in a cooperative enterprise in close geographic association and with the same patient groups. Perhaps in the nonstate institutional setting the valuations of psychiatrists reflect a more independent and competitive nature of the professional relationships. The competitive nature of the relationship between noninstitutional psychologists and psychiatrists is discussed elsewhere (Rettig, Jacobson, & Pasamanick, 1958c; Zander et al., 1957).

Clinic psychologists appear to have greater job satisfaction than state institution psychologists. The general trend is statistically significant in only one particular case, but the five-item scale, we believe, is a rather crude one and particularly so considering the sophisticated groups in this study. Clinic psychologists clearly believe that the people they work with are more satisfied with their work than do the institution psychologists. This particular aspect of job satisfaction appears to be a more sensitive indicator, perhaps because of its "projective" qualities since, for all groups, this item manifests the greatest difference, consistent with general trend and in the predicted direction (Rettig et al., 1957).

The psychologists, compared with the other professionals in this study, are high in objective status and income, exceeded in both cases only by psychiatrists. However, their relative youth (nurses

were the only groups which were younger) adds some emphasis to the ranks of their income and objective status among the hierarchy of the professions, since these generally increase with age. The job satisfaction of psychologists which is the lowest of all professional groups requires explanation.

The patterning of the factors of job satisfaction stated in degrees of importance might be seen as reflections of values, particular to psychologists. For example, it is probably not sophisticated or even socially acceptable for psychologists (or any professional person) to value the "patient's respect" because of the status and authority differential implied and because of prohibitions on utilizing relations with patients to gratify one's own needs. A psychologist may not be likely to admit the importance of "security" in view of popular conceptions of security seeking (dependence!), a clearly unacceptable characteristic, tolerable only on the patient's side of the desk or couch. The premium placed upon "freedom in one's work" may be, not only the opposite of disdain for "security," but also an indication of the instrumental necessity of freedom in psychological work. "Regular work hours" is a rather petty desideratum compared to the other factors, and one that would not square with self conceptions as dedicated scientific or professional persons. It is easy perhaps for psychologists to regard "regular work hours" as unimportant since the possibility of working shifts or irregular hours is probably more remote for psychologists than for other professional personnel in this study.

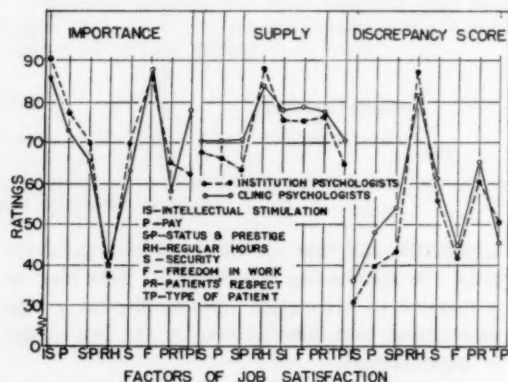


FIG. 3. Patterns of importance, supply, and discrepancy scores on the factors of job satisfaction for institution and clinic psychologists.

The noteworthy low job satisfaction of psychologists may be illuminated somewhat by the general observations that psychologists experience a low supply of those factors which are regarded as important ("freedom in one's work," "intellectual stimulation") and a plentiful supply of what is not valued ("regular work hours"). The factors which are valued by psychologists ("freedom," "intellectual stimulation," "type of patient") can be regarded as rather nonmaterial factors of no practical and personal advantage. We consider psychologists then as very intellectual and idealistic, compared with the other professional groups in this study. We believe that the psychologists' backgrounds are more academic and theoretical and compared with psychiatrists, nurses, and perhaps social workers and teachers, psychologists have less fixed assignments, less delegated authority, less of a burden of immediate responsibility in the performance of their duties. Psychologists' activities are characterized by a greater balance of thinking as opposed to doing. Perhaps the lack of well-defined duty assignments and, too, their training in objectivity eventuate in considerable feelings of detachment, lack of very personal involvement; psychologists express more readiness to move than most groups. Moreover, a lack of attachment, psychological and organizational, would appear to hinder the extraction of material satisfactions and social rewards—"pay," "status and prestige," etc.—and these factors are significantly related to the job satisfaction of psychologists. Other groups may derive their job satisfaction from other sources, but for the psychologists, "intellectual stimulation" appears to be most essentially related to job satisfaction.

The lack of "status and prestige" in the psychologists' subjective experience and the direct relationship between this factor and psychologists' job satisfaction is noteworthy. We believe that the need for recognition is of much significance in the motivational structure of professions and have dealt with this more thoroughly elsewhere (Rettig, Jacobson, & Pasamanick, 1958b).

When we compare the clinic psychologists with the institution psychologists, we find fewer significant differences between them than between any other pairs of professional groups. A consideration which emphasizes this similiarity is the varied sources from which psychology subjects are selected. The variability is greater only in the

case of the private practice psychiatrists. Furthermore, the differences are in the same direction as is generally found between total state institutional and total nonstate institutional samples, i.e., the noninstitutional personnel have higher objective status, pay, and are younger. Thus psychologists are more homogeneous, their occupational values, status perceptions, and job satisfaction apparently less affected (or selected) by the particular occupational type of work setting than psychiatrists, teachers, etc. We are tempted to say that psychologists are surprisingly alike and remarkably unaffected by objective reality, but that would be oversimplification.

In comparing clinic psychologists with institution psychologists, a number of findings are difficult to explain. One is the homogeneity mentioned above. The clinic psychologists attach significantly more importance to the "type of patient" than the institution psychologists do. Perhaps an institution exercises less control than a clinic over the selection of patients and then the resident population requires more routine care, limiting both choice and satisfaction, relatively. Perhaps the institution psychologists lose interest, relatively speaking, in the matter of different types of patients, an apathetic reaction in the face of an absence of control over choice.

The findings indicate that perhaps the institution psychologists are relatively unconcerned with status accorded by the public; they might seek their status satisfactions from within their profession and their colleagues in related fields, possibly from within the institution, staff, and patients. The clinic psychologists appear to be unconcerned with the status attitudes of other psychologists in general and institution psychologists in particular. Pushing into the realm of speculation, for our findings give no clear indications—perhaps because our methods could not tap this area—the clinic psychologists may be more concerned with successful competition in an open field where success and personal achievement are valued, an orientation more consistent with our general cultural ideals of achievement. The institution psychologist may be oriented more toward ideals of dedication to public service and applied science.

We believe that the clinic psychologist probably has a better chance of attaining higher status in the community at large and that on the other hand the aspirations of the institution psychologists not

so readily attainable are likely to suffer from stultification.

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## Comment

### Researchmanship or How to Write a Dissertation in Clinical Psychology Without Really Trying

1. Skim through the paragraph headings and summaries of articles in the last couple of issues of the *Journal of Abnormal and Social Psychology*, *Journal of Consulting Psychology*, the *Journal of Projective Techniques*, and *Journal of Personality*. This is known as *surveying the literature*.

2. Select the measurement device most frequently used in recent work (preferably one that is self-administering and requires no effort to score or interpret, like the Taylor Manifest Anxiety Scale). This assures your study of *timeliness, topical relevance, and publishability*.

3. Find some other fashionable instrument or procedure with which the first has never been correlated, also preferably one that gives a quantitative, objectively derived score. State that this score will be adopted as a measure of *self-actualization*, or some other fine sounding and not too easily defined concept, and note that, when you say "self-actualization," you mean it only in the restricted sense of your test's score. This puts your work on a sound *operational basis*.

4. Get a group of subjects—any old subjects who happen to be available. Divide them into an experimental and a control group, taking elaborate precautions to match them for mean age and years of schooling, whether these variables have anything to do with your problem or not. This procedure solves all of the problems of *experimental design*.

5. Give the two tests to both groups, scaring hell out of the experimental group by solemnly assuring them that your dependent variable is a test of intelligence, leadership, innate decency, and sexual potency and that they have all scored below second percentile and you are sorry but you feel honor bound to report them to the dean. This process, called *experimentally manipulating the independent variable*, is really good clean fun because after the papers are handed in you tell them that it was just a little trick, all for science.

6. Then you correlate *X* with *Y* under your experimental and control conditions, reporting the coefficient and its *PE* to four decimal places. This shows you to be a *rigorous, mathematically exact* sort of scientist.

7. If your correlations are not quite different enough, rescrutinize the data to make sure that you have not inadvertently included subjects who were *insufficiently*

*motivated*, and eliminate the ones who were fouling up the results.

8. Now write it all up, predicting whatever finding you happen to get, which makes it permissible for you to use one-tailed tests of significance throughout. Make it as long, ponderous, and dull as possible; dedicate it jointly to your spouse and your advisor, "without whose help this work could not have been completed," and you are in.

ROBERT R. HOLT  
New York University

### Uniformity of Style in Professional Writing During the First Century

As one concerned with teaching scientific writing to graduate students in psychology, the present writer has advocated strongly (*Science*, 1958, 127, 1458-1459) the uniformity of style expressed in the APA's *Publication Manual*. The elimination of idiosyncracies and carelessness from term papers and theses certainly is as desirable as their absence in professional journals and provides good training for the preparation of articles for publication.

To graduate students, however, APA style seems to represent, among other things, stultifying rigidity, the height of anxiety reducing compulsivity, the severity of a harsh superego, and the epitome of the *D<sub>a</sub>* personality. As is apparent, these petulant remarks have been uttered by students in clinical psychology classes. However, such emotions are probably shared by experimentalists and statisticians as well as by their more qualitatively minded colleagues.

Many graduate students have expressed the further thought that style manuals are a new straight jacket for insuring modern conformity and extinguishing the creative spark of individuality. This thought is incorrect entirely.

The exactness demanded in professional writing and the absolute assurance of the advantages of uniformity in style can be traced back to the extraordinary care with which Jewish scribes transmitted their sacred writings. In talmudic works the most exacting procedural rules were established, determining for all a uniformity of style. Between A.D. 90 and 100, a synod, convened at Jamnia, established instructions in various matters of form. A list of these standardized procedures has been presented by Allegro in his *The Dead*

*Sea Scrolls and the Origin of Christianity.* By the end of the first century the following procedures were fixed: type of script (cf. APA manual, Section 9.16: *Type no word in all capitals.*); ruling of the page (cf. APA manual, Section 5.24: *Rule completely with light pencil lines*; and 5.25: *Make no heavy rules.*); size of columns (cf. manual, Section 5.71: *The dimensions of a table.*); gaps between words and sentences (cf. manual, Section 9.15: *Double space throughout.*); and even the color and nature of the ink (cf. manual, Section 9.11: *The typewriter ribbon should make a dark, even impression.*). As can be seen, the similarities between considerations of the style of professional writing in the first century and the twentieth are close.

In addition to removing a suspect recency from the stylistic reinforcement of the APA Council of Editors, additional comparison reveals the leniency of this group. For they break tradition. In the first century the clothing to be worn by the scribe was also set forth immutably. A diligent search of the APA *Publication Manual* has indicated no consideration of this topic.

Awareness of these interesting parallels, and especially the nonobservance by psychological stylists of one, may bring a small measure of comfort to the otherwise troubled hearts of neophytes in the intricacies of psychological writing.

DELL LEBOW

Richmond Professional Institute

### The Shortage of Journal Space

Are psychologists unduly handicapped because of limitations in quantity and type of journal space? Should more APA money go for journals? Should state and regional associations sponsor new journals?

Repeating studies is one important aspect of science. There are enough difficulties involved in repeating them, in psychology, without adding publication bottlenecks. As a matter of fact, there is a need for greater speed, and space, when repeating is so difficult. If something new is suggested by one study, it should be possible for it to be put to the test and then the profession informed of the results of these follow-ups. As things stand, years could go by, from the time of the submission of the first study to the appearance of new

reports, showing how it stood the test of being repeated under a variety of conditions.

The way many of the journals are setup, and in spite of the "letters to the editor" provision sometimes found, there is little opportunity to point out implications, shortcomings, and even grievous errors. Many times, a rapid exchange of opinion and the association of various research findings is very appropriate, if not a dire necessity; however, there is little provision for this.

It is suggested that the APA sponsor two new journals to be entitled *The Psychological Forum* and the *Psychological Clearing House*. They would not be subject to the rules that govern research reporting in most APA journals: the editorial policy would be to give maximum freedom to the authors. These new journals would help to stimulate progress in psychology.

JAMES W. RUSSELL

Northwestern University Counseling Center

### Whither the Convention?

Do we have to meet every year during the Labor Day weekend and usually in the hot over-sized cities that can accommodate us? I am aware that polls indicated that the majority of APA members prefer the Labor Day period. I am also aware that our size forces us to meet in a relatively few cities.

However, I should like to suggest that we try varying the time of the meeting, say every three years. Suppose we meet in the month of October. We could then widen the possibilities for locales. Atlantic City could handle us, I believe, provided we avoid holidays. There may be other resort areas that would welcome us in their off season. I, for one, would welcome them.

Is there a valid reason why we must meet at the same time each year? Would not some variation in the time be a more democratic procedure in that it would give many members an opportunity to attend an occasional meeting? I am thinking of those who cannot come during the summer.

In all events, I pass this suggestion on to the membership. My hunch is that this proposal would be bought by the membership if they had an opportunity to vote for it.

HARRY B. GILBERT  
New York City

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# Psychology in Action

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## HUMAN QUALITY CONTROL IN NAVAL AIR TRAINING

JAMES R. BERKSHIRE

*United States Naval School of Aviation Medicine*

AND VERNE W. LYON

*Staff, Chief of Naval Air Training*

TRAINING programs resemble industrial production processes. A raw material is selected in accordance with some minimum standards of quality and put through a series of processings which change it progressively, in specified ways, until a desired end product is reached. Usually, at several levels of the sequence, the product is measured to determine the probability that the end product will be within the quality tolerances necessary for its market. In industry these measurements may be made by gauging, by chemical or mechanical tests, or by visual or electronic inspections. The specification of the tests, the points in the process at which tests are to be made, and the minimum allowable standards on the tests are the province of engineers. The procedures are commonplace—they are part of what is known as product quality control.

In training programs, whether these be military (aviators, technicians, submariners) or civilian (engineers, physicians, teachers), similar quality control requirements exist. Candidates for the training must be screened to eliminate those who are likely to fail in training; minimum training standards that will prevent the graduation of potential job failures must be imposed at appropriate levels of the training process. This is human quality control; and, since the problems involve the measurement of the potentials of people, it would appear to be the natural province of psychologists.

Typically, however, training programs have not been conceptualized in quite this manner. The responsibility for selecting people who will be able to complete the training program has been delegated to psychologists. The responsibility for establishing valid minimum standards within the program has not.

This paper describes an important instance of the application of psychological research methods to human quality control over a total training program. The training program is that for naval aviators. The re-

search organization is the Aviation Psychology Laboratory of the School of Aviation Medicine at Pensacola, Florida. The laboratory, as such, has been in existence since July of 1951. Its research activities are extremely diverse, ranging the field of psychology from animal learning to human engineering. The laboratory is staffed by a nucleus of 4 or 5 civilian research psychologists, augmented by 8 to 12 officer psychologists who have recently completed their degrees and are either fulfilling their military obligations<sup>1</sup> or are career officers.

In the early days of the laboratory (and in the aviation psychology programs that preceded its establishment) the major concern was with selection research. The first study which held potential utility for within-training quality control was that of Hollander in November of 1952. He found two preflight academic courses, Navigation and Aerology, in which low grades indicated three times the normal probability of later flight failure. He made a series of recommendations for the use of these grades in guiding administrative decisions as to whether students who were in difficulty should be dropped or given another chance. Hollander pointed out that the early identification and elimination of potential flight failures would result in a substantial reduction in over-all training costs. Hollander's recommendations, however, were apparently culturally premature. It was not until two years later, after publication of a paper by Poe and Lockman (1954), that procedures similar to those advanced by Hollander were adopted. Poe and Lockman used multiple correlation methods to identify the best combination of selection test scores and preflight course grades for the prediction of subsequent attrition from training. A weighted combination of Mechanical Comprehension Test score, physical training grade, preflight ground

<sup>1</sup> Information concerning officer billets in the Naval Aviation Psychology Program may be obtained from Aviation Psychology Branch, Bureau of Medicine and Surgery (Code 513), Department of the Navy, Washington 25, D. C.

school grade, and officer-like-qualities rating yielded a so-called "Pre-Flight Progress Grade." It was shown that half of the students whose progress grade was below 35 (lowest 7%) were potential flight failures or would voluntarily resign. Those who did graduate required three weeks more than the average student and had final over-all grades that were 1.3 *SDs* below the mean of the graduates with higher progress grades. The progress grade was computed at the end of preflight school and used thereafter in training only if the student was in difficulty and being considered for elimination.

Now the use of within-training screening devices developed in this manner, while demonstrably profitable, has certain logical weaknesses. First, the ultimate validity of the device used is contingent upon the validity of the training failures it predicts. Failures may take place in practically all phases of training. If the minimum standards in these phases are arbitrary, as is typical, they may be too high (be failing men who could succeed on the job), too low (be passing men who will fail on the job), or inappropriate (performance in the particular phase of training has no relation to job success or failure). In these cases, devices such as the progress grade may merely aid the administrators in arriving more expeditiously at the wrong decisions. Second, there is a possibility that such a device will confirm and increase the effects of existing biases. For instance, if a number of administrators are convinced that mechanical aptitude, as represented by scores on the Mechanical Comprehension Test, is critical for job success, then low MCT scores may make it more likely that a student in trouble will be dropped. Any study conducted with these student failures as part of the criterion group is quite likely to find that MCT score predicts training failure. It may, however, have no relation to job failure.

Similar hazards are present in the practice of basing selection and procurement standards solely on probability of training failure. If the minimum standards in training are inaccurate or erratic, the selection procedures may be accepting potential job failures or rejecting potential job successes.

In order to guard against these possibilities, human quality control in a training program should start with identification of graduates who have been unsuccessful on the job and work backwards to the determination of those minimum standards which will prevent the selection and/or graduation of potential failures. Therefore, early in 1956, Berkshire and Lyon (1957) visited 90 fleet squadrons to identify such unsatisfactory graduates. Specifically, squadron commanders were asked to name men who, in their opinion, should not have graduated and to indicate whether the man's inadequacy was as a pilot, as an officer, or both. In ad-

dition the names of all graduates who had been killed in pilot-error accidents were collected. Analysis of the various selection test scores and training grades of unsatisfactory men, as compared with those of a fleet satisfactory sample, showed that grade in presolo flight training (a man's first 19 flights) provided one of the most effective minimum standards. Of the lowest 7%, in terms of presolo grade, half failed to graduate. Of those who did graduate, half were unsatisfactory or killed in the fleet. It was shown that dropping the lowest 7% immediately would result in an annual saving of approximately \$7,000,000 in training costs and a reduction of about 20% in the number of unsatisfactory men reaching the fleet. A study of the accident records of these same men revealed (Berkshire & Gallagher, 1956) a pilot-error accident frequency that was twice normal, both in training and in the fleet. As a result of these findings appropriate minimum standards in presolo training were put into effect in early 1957.

One year after the first visits to fleet squadrons a second set of visits was conducted. This time, in addition to identifying unsatisfactory graduates, squadron commanders were asked to identify those first-tour pilots whom they considered to have outstanding potential as career officers. The results were compared by procurement source, selection test scores, and training grades. A number of ways of increasing the proportion of outstanding officers through changing procurement sources, raising selection test minimums, and raising certain minimum training standards were proposed (Berkshire, Ayres, & Ray, 1958). Since the men covered in this survey had graduated prior to the installation of the presolo minimum standard based on the previous survey, it was possible to check and confirm the results of the earlier study. (Once a minimum standard is put into use it is no longer possible to check on its validity. Thus, for any minimum standard to be adopted, a high degree of confidence is necessary.)

In addition to the usual grades, experimental peer ratings of officer potential, made by the man's section-mates, were available for many of the men covered in the second fleet survey. Investigation showed that of 79 men whose peer ratings were in the lowest 7%, only 31 graduated. Thirteen of these had been assigned to squadrons that were visited. Only 2 of the 13 were said to be satisfactory in the fleet (Berkshire & Nelson, 1958). These results were corroborated by a study (O'Connor & Berkshire, 1958) covering a later sample, in which it was found that 61 of 77 men with peer ratings in the lowest 7% had attrited from training. Based on these findings, it was recommended that a policy be adopted of immediately dropping any man who had a peer rating below 35 if he encountered training or disciplinary trouble of any kind.



Comparisons of the training performances of men who were unsatisfactory in high performance jet squadrons with those of men who were satisfactory in squadrons equipped with propeller type aircraft led to a recommendation that assignment to type of training (jet or prop) be based on grades in presolo flight and preflight ground school. It is anticipated that this will result in further substantial reduction in the number of fleet unsatisfactory pilots and in fleet casualties.

Based on the various studies of the relationship of selection test scores and early grades and ratings to later failure in training and in the fleet, a manual for training administrators was developed (United States Navy, 1958). This manual shows, for each score level on each measure, the probability that a man will graduate and the probability that a graduate at that score level will be successful in the fleet. The manual is used to assist in decisions about failing students. In order that the manual may be kept current (and to provide data for semiannual revision of grade conversion tables) procedures have been established which provide for routinely punching all student data into IBM cards at the end of preflight training, at the end of basic training, at the end of advanced training, or at any point at which a student may attrite.

Present findings indicate that minimum standards already installed and under investigation should reduce the number of unsatisfactory men reaching the fleet by about 50%. One might think that further quality control efforts would soon reach a point of diminishing returns. But, as higher performance aircraft are introduced into the fleet, it can be expected that the training program will be changed. Further, increasingly high aircraft performance may demand increasingly able pilots. Thus the requirement for study of the minimum training standards is a continuing one. Under the sponsorship of the Chief of Naval Air Training, fleet squadrons will be visited annually to provide criterion data for such studies. The procedures for translating these data into an effective quality control program may be summarized as follows:

1. Identify satisfactory and unsatisfactory graduates.
2. Examine training variables to identify areas of training which are critical for job success. Examine training grade distributions to locate cutting scores

which will result in the early elimination of potential job failures.

3. Examine selection and procurement variables. Determine effects of possible new minimum selection standards and of changes in procurement sources on proportions of expected training and job failures.

4. Conduct research to improve the technical adequacy of training grades.

5. Develop procedures and devices to facilitate administrative use of the minimum standards identified.

It is the writers' conviction that such quality control procedures could be profitably applied to nearly all training programs.

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## Psychology in the News

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### *Into Darkness . . .*

Readers outside the New York City area may not realize what a fuss was kicked up by Ed Murrow's CBS show discussing the role of "call-girls" in big business.

New York City police officials became hysterical over the "revelation" that businessmen actually, and that some of the city's women were no better than. The furor was larger, but was otherwise identical, to the uproar one sometimes sees in Blue Earth, Kansas, say, when the *Bugle* prints what everyone knows: that *there are slot machines at the Legion clubhouse, and they gamble on Sunday.*

The producer really responsible for "The Business of Sex" is Irving Gitlin, Director of CBS Public Affairs. He attended City College, Columbia, Harvard, and MIT (the latter two for radar training during the war). He has always had an interest in sociology and psychology and says this program was *not* about sex, but "about the social ethic of our society . . . the way we value each other . . . our capacity to treat each other merely as commodities."

Gitlin was responsible for the CBS documentary on mental illness "Out of Darkness," a program which mixed up the Meningers and Orson Welles and schizophrenia in a strange and wonderful way. Gitlin and his office plan several more documentaries to call "a spade a spade," supposing that they survive the present strumpet fanfare.

### *Murder Most Foul . . .*

Television, a medium of sensory perception, seems always to be dabbling with other media—or perhaps, in this particular instance, medium is the correct word.

Perhaps by the time you read this, the program will be dead; but, if it is not, look out for one called "Alcoa Presents." In this, John Newland presents "true" stories of ESP, "levitational phenomena," etc.

The first one, which this reviewer saw, was "a tale of possession" (namely, a living person was for a time inhabited by the personality of a dead person). In the words of John Crosby, the TV critic:

A young couple is on their honeymoon, the bride being a real sugar-mouthed Southern belle, who falls asleep on her husband's shoulder and wakes up talking pure Yankee. "Take your hands off me," she snarls at her husband, and lopes off into the night. "She looked so different," mutters her husband to the psychiatrist, who is brought in on the case. And she sure did look different. She had a whole new personality and the name of a dead girl who had supposedly committed suicide. "I didn't kill myself!" the girl screams. "I was murdered! I was murdered!"

The host, John Newland, at times gave a shrug as if to encourage the weissenheimer type of viewer to feel that no inside dopester really believes what is said about the stories being true. Off-camera, Newland has stated:

Originally the concept was that the stories be absolutely true but this proved to be difficult. Now the stories are just BASED on fact. We plan to use no stars. By using unfamiliar faces we hope to gain acceptance.

The "psychiatrist" in the dramatization told the distraught husband that the whole thing was out of the domain of psychiatry. Psychologists were not mentioned, but there will be other stories.

Sometimes we think that the personality of radio is now possessing television to tell us something: "I was murdered" is the message we seem to hear.

### *Of Bell and Buttons . . .*

Push-button phones? Bell Laboratories now reports that one could make more compact phones, more convenient to operate, by using all numeral numbers and push buttons. Tests indicate few customers would object—but all numeral numbers would be harder to remember. This change is not planned. But if it ever happens, it will stem from studies done by a psychologist with Bell, namely, John E. Karlin, who received his MA at the University of Cape Town and his PhD at the University of Chicago.

### *Needed: Patients Without Patience . . .*

*Hospitals are run for the convenience of the doctors, nurses, and staff; the patient's needs come last.*

These are the exact words of the headline on the main article in *Look* of February 3, 1959. The

entire article (by Roland Berg) is devoted to an extremely critical survey of hospital management. Both the survey and the article concentrate on the certain hospital and physicians' practices which are believed to cause harmful psychological effects.

There is a startling story here, and one interesting aspect of it is that this survey was initiated by the medical profession. The whole project was started when the California Medical Association sought to find out why people sue doctors. A team of researchers was commissioned by the medical association to study all factors relating to malpractice claims. And certain psychologists and sociologists were then hired to use their respective skills; the final report was largely written, we understand, by nonmedical persons. And that led the investigating social scientists to hospitals and ultimately to conclusions like these, as quoted from *Look*:

The interviews revealed that many of the individuals concerned, both physicians and patients, were emotionally ill. . . . It became clear that psychological reactions are more important factors in malpractice lawsuits than the quality of treatment or whether the doctor makes a mistake in medical judgment. . . . The emphasis on staff convenience rather than patient benefit stems from the faulty organization of hospitals, the investigators point out. Most hospitals suffer from a lack of over-all authority. . . . Most frequently, the investigators found, the members of the medical board and the doctors on the staff are responsible for the breakdown of authority. This may be due to the average physician's reluctance to submit to discipline. Usually, he regards himself as a free and independent individual, not bound by ordinary rules. As one administrator sought to explain it, "It's as simple as putting up a No Smoking sign in the elevators. Everyone else in the hospital will respect the rule except the doctors; they continue to smoke."

The report was not destined for publication, and only a handful of copies were printed. One of them, however, found its way to a San Francisco science news writer. From West Coast publications it went to *Time* Magazine, and now in *Look* it receives an extended treatment. A key psychologist on the project, according to *Time*, was an APA member.

The report itself concluded with urgent recommendations for better psychological training of hospital personnel and for research and experimentation in hospital management. *Look* gives, among others, these quotes from the report:

It is recommended that every support be given to the medical schools in their programs of research to explore these important patient reactions, and in their programs of teaching psychological medicine to physicians and nurses.

The elaboration of such teaching programs is very much in order. . . . Since there is reason to believe that conventional hospital procedures may produce latent dissatisfaction and/or psychological or physical damage to patients, it is strongly recommended that support be given to experimental programs for the revision of traditional hospital organization.

The surveyors thought that people ought to cause more trouble in the hospitals. . . . Every patient bears responsibility for seeing that he gets good health care by means of thoroughly unsubmitive and firm demands upon the medical personnel who are paid by him to provide for these very needs.

And for another unsubmitive approach, read the February *Ladies Home Journal* for a blast on the subject of illnesses your children may pick up in hospitals. This writer, it might be said, also reads the *Journal of the American Hospital Association* and can testify these issues are not unknown to hospital administrators who are trying to do something intelligent about them.

However, it seems that while doctors read these things, they are not too overwhelmed by words. Unless some real power is brought into play, one can only hope for the influence of words and facts upon some people, but that hope for change remains positively pathetic. And that is similar to what we rhetoricians call the *pathetic positive*, as in some signs which we have seen in elevators: POSITIVELY NO SMOKING.

*Invisible Us . . .*

In *Harper's* of February 1959, there is an article called "New Hope for the Mentally Ill" by John Bartlow Martin. Most psychologists familiar with clinical problems will probably feel this is an excellent article. However, since it places particular emphasis upon the need for research, psychologists may wish that Martin, widely and justly known for the accuracy and completeness of his work, had researched long enough just who it is who mainly does research in this area. The article makes no reference whatever to psychology or psychologists.

Angry readers are requested not to write to the *American Psychologist*, pointing out what psychologists do in studying tranquilizers, studying human beings, working in clinics and state hospitals, etc. Send your information to Harper and Brothers; they are the ones who seem uninformed about the psychiatrist and his brothers. The address is: 49 East 33rd Street; New York 16, New York.

—MICHAEL AMRINE

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## Psychology in the States

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### State Associations in the Space Age

During the last APA convention, the program committee of CSPA had made so bold as to schedule a workshop entitled "The State Association in 1970." The panelists had made even bolder by accepting the invitation to play variations on this theme. Whether as latter-day Huxleys or Orwells, plain Drew Pearsons, or even plainer psychologists, the crew of four made a variety of predictions—with impunity, maybe even with impertinence.

Among the welter of predictions—none of them, happily, very dire—were some relating to the inevitability of growth and its concomitants, the changing complexion of the relation between the state associations and their parent organization, the bridging of the gap between the academic and the professional man, and the contrast between the problems faced by the Davids and the Goliaths of state associationdom.

One of the panelists (perhaps a co-editor of this column, who is willing to plead guilty to myopia should the predictions run afoul of future realities) ventured a series of guesses. They were these.

1. Individual differences, while they continue to persist, will by 1970 have become less pronounced. 1959 finds them much in evidence. Today one association feels an avant-garde mission, another is content to wait watchfully; one is brash, its neighbor deferential; one has need to flex its muscles, another hardly minds playing the 98-pound weakling (indeed, feels some security in the little brother role). Operationally, the differences are reflected perhaps in such facts as a monthly (even glossy) newsletter in one quarter, a once-a-year mimeographed sheet in another; one seeks diplomatic immunity in a series of unadulterated scientific paper-reading sessions, the other does not think twice about entering the desegregation fracas. Without exercising gratuitous value judgments, the prediction is that by 1970 the differences will be much less striking, though hardly absent.

2. Vis-a-vis APA, state associations will increasingly feel more like peers, less like dependents. A

decade ago—this argument would run—the state associations were in their infancy; the recent CSPA-BPA era has witnessed the ambivalence of adolescence in the parent-child relationship between mother APA and its 52 offspring; 1970 will find the state associations having come of age, hopefully not regarding the parent as in her dotage, while themselves feeling the security of having lived yet another decade.

3. What transpires at the state association level will always reflect in microcosm what happens at the national level in macrocosm. The state association stage is a bit smaller, its trappings less elaborate, the *dramatis personae* fewer. But the plot (if this is not a loaded word) is the same as that which plays itself off on the national stage. And the analogy will still hold by 1970.

4. Come 1970, the state associations will seem less like geographical subunits of APA, more like convenient laboratories for the study of forces acting on psychology as a whole. As such, they may well be the testing grounds where psychology can experiment with a variety of societal roles, internship agencies in which psychologists learn to translate the messages of psychology to the community.

If all of this be sheer conjecture—and it is—make the most of it, one might add.

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**Quo Vadis.** A well-worn Washington adage has it that people who work but two blocks apart are fated to meet not in a downtown bistro but at a human engineering conference in Hawaii or a human relations workshop in St. Louis. Could be that our more traveled members frequently are carried by other business to cities in which, again two blocks away, the state association is holding its meeting, unbeknown to our visitor who might otherwise have attended, perhaps even participated.

By way of handling the problem at least partially, state associations were recently requested to submit a schedule of their projected meetings. The majority obliged; their replies, more or less specific, indicated the following:



## SCHEDULE OF STATE ASSOCIATION MEETINGS

ASSOCIATION	DATE	PLACE
Alabama	May 9	Montgomery
Arizona	April	University of Arizona
California	April 16-18	San Diego
Colorado	April 4-5	Aspen
Connecticut	April-May	(Undecided)
Florida	April 23-25	St. Augustine
Hawaii	May-June	University of Hawaii
Illinois	April 10-11	Champaign
Indiana	April 11 or 18	Indianapolis
	Nov. 14	Indianapolis
Iowa	May 23	Grinnell College
Kansas	April 16-18	Washburn University
Louisiana	March 13-14	New Orleans
Maine	March	Nasson College
	May	University of Maine
Massachusetts	March 25	Harvard University
	April 25	Clark University
Michigan	March 27-28	Michigan State University
Missouri	April	Columbia
Nebraska	April-May	Lincoln
Nevada	May 16	Tonopah
New Hampshire	May 2 or 9	Colby Junior College
New Jersey	May 2	(Undecided)
New Mexico	March	Albuquerque
	June	(Undecided)
	September	(Undecided)
New York	May 8-10	Ellenville
North Carolina	May 2	(Undecided)
North Dakota	April 17 or 24	Bismarck
Ohio	April	Columbus
Oklahoma	March	(Undecided)
	October	(Undecided)
Oregon	May	(Undecided)
Pennsylvania	May 8-9	Pittsburgh
Tennessee	November	Nashville
Utah	April 25	Utah State University
Vermont	May 9	(Undecided)
Virginia	May 8-9	Charlottesville
Washington	May 8-9	Seattle University
Wisconsin	May	University of Wisconsin

**Of Sheep and Goats.** Several recent actions of the APA have reflected a determination to insure psychology its rightful place in the social and scientific enterprise; it has, in the same breath, reiterated the need, indeed the responsibility, for keeping our own house in order. State psychological associations seem no less conscious of the importance of the latter. At least, their governing instruments contain specific regulations providing for termination of membership on a number of counts. Analysis of their constitutions and by-laws discloses the following grounds and related procedures:

**Nonpayment of Dues.** The most common practice here is to regard nonpayment of dues for two consecutive years as equivalent to resignation. Several associations apply the

more stringent one-year rule, while one actually sets a six-month limit. In most cases there is provision for reinstatement after dues in arrears have been paid.

**Inappropriate Behavior.** Commonly, membership may be terminated for:

1. "conduct injurious to the Association, its reputation, or its purposes"

2. "unprofessional and unethical activities"

3. "acts bringing the field of psychology into disrepute"

While details for dealing with such cases vary from state to state, the following represents a *composite* of the most commonly employed procedures:

1. Initiation of charges by a minimum of  $x$  members (usually from 2 to 5)

2. Submission of specific charges in writing (to the executive body or a specified officer)

3. Referral to a Committee on Professional Practice and Problems (or appointment of a special investigative committee)

4. Decision by the committee as to whether to drop the charges, allow opportunity for resignation, or refer the case to the executive body for action

5. Notification of the member concerning charges pending

6. Hearing of the member at a joint meeting of the executive body and its committee, with opportunity to present his case in person and/or to be represented by counsel

7. Decision by the executive body and its committee

8. Right of appeal to the membership by the defendant

9. Secret ballot by the membership (sometimes at a closed session) to approve or reverse the action of the executive body and its committee (usually by 2/3 vote of the members present at a duly constituted meeting)

**Out-of-State Residence.** While in some states (e.g., Maine) members who leave the state may continue in Affiliate status (upon payment of dues), others (e.g., Michigan) automatically terminate membership when the person takes up residence outside the state.

**Legislation—Chapter Three.** The third in the tripartite convention workshops on legislation has been summarized by Thomas M. Magoon, who served as Chairman with the assistance of Frank Auld, Jr. and Jane D. Hildreth. As seen by Magoon, the session pointed up issues such as the following:

1. Legislation is easily enough entered into, not nearly so easily gotten out of.

2. Legislative involvement spells a complex series of relationships, interactions, and evaluations, many of them relatively new to psychologists.

3. The winning of legal recognition brings with it certain benefits; but greater visibility is not without its price, and more visible objects make better targets.

4. The decision to seek legislation has its impact on the state association itself. Unless the effects are carefully taken into account and judiciously handled, the subsequent victory may be a Pyrrhic one.

The import of much of the above is contained in a number of publications readily available. The workshop saw the following as among the required basic reading:

Regulation of Psychological Counseling and Psychotherapy. *Columbia Law Review*, 1951, 51, 4.

Occupational Licensing Legislation. Council of State Governments, 1952.

State Regulation of Psychologists. Illinois Legislative Council Report, 1956.

CSPA "Legislative Surveys of State Associations and State Boards of Examiners," 1958.

Legal opinion (David Louisell) regarding the distinction between Certification and Licensure. APA Committee on Relations with Psychiatry, 1958.

APA Central Office Legislative Packet (including present bills, steps in planning).

"Joint Report of the APA and CSPA Committees on Legislation." *American Psychologist*, November 1955.

General issues discussed, says Magoon, included the role of legislative committees, attitudes of psychologists, relationships with professional and community groups, characteristics of regulatory bills, orientation to state legislatures and the legislative process, and public hearings before state legislative committees.

\* \* \*

**Operation Friendship.** For the eleventh time in as many years, Mental Health Week will be observed this spring on a nationwide scale. The week of April 26-May 2, 1959 has been so designated, and the attendant projects will be directed by the National Association for Mental Health and co-sponsored by the National Institute of Mental Health of the United States Department of Health, Education, and Welfare.

Theme of this year's observance is "Operation Friendship," a project intended to bring to the nation's mental hospitals the biggest possible influx of visitors. The purpose, a threefold one of: (a) making visible to patients the interest and concern of their more fortunate fellows, (b) promoting clearer understanding of the mentally ill as people, (c) educating the public regarding recent developments in care and treatment as well as imparting a more sanguine attitude toward rehabilitation.

APA has been requested by NAMH to cooperate in helping make Mental Health Week a meaningful event. The assistance of state associations has been sought in particular. A roster of officers has accordingly been submitted to NAMH; the latter will reciprocate. The hope is that representatives of both associations will find a number of meaning-

ful activities which can be carried out jointly at the state level.

**Meeting Extraordinaire.** Seldom do our state association meetings close with a "*vin d'honneur*"; seldom, indeed, do they match, let alone surpass, in variety and comprehensiveness the Annual Meeting of the Ontario Psychological Association held in Ottawa on February 6-7 and hosted by the Ottawa Association of Psychologists.

From its keynote address on "The Emergent Role of Psychology" to its final session on "Delinquency, Crime, and Reform," the program ran the gamut of contemporary issues in which psychology has a stake. Engineering psychology figured, as did also market research and human relations training. Educational psychology merited a session of its own, while mental health, both here and in the perspective of the World Federation for Mental Health Congress in Vienna, likewise received its share of attention.

The themes were hardly all standard. A panel on "Traffic Safety" found members of the Research Committee of the Ontario Department of Transport participating; another session heard "mental health implications of uprooting and resettlement" discussed. Problems of legislation were not slighted (especially since plans are under way for submission of a bill to the Provincial Legislature), while a business meeting kept the group mindful of problems of profession.

**Twenty-Three Skidoo.** We cannot resist this final lead which is inappropriate except for the fact that (a) the number 23 seems unrelated to any thing else and (b) it represents the current total of state association representatives named to the APA Council of Representatives. Eighteen had been listed in the December issue of the *American Psychologist*; the following five have since been designated:

Joan H. Criswell . . . . . District of Columbia  
Harry V. McNeill . . . . . New York  
William Schofield . . . . . Minnesota  
William J. vonLackum . . . . . Tennessee  
Walter L. Wilkins . . . . . Missouri

—J. G. DARLEY  
Chairman,  
Board of Professional Affairs  
E. L. HOCH  
Administrative Officer,  
State and Professional Affairs

## Notes and News

The American Board of Examiners in Professional Psychology, Inc. publicly expresses its appreciation for the services of its Diplomates who served as readers of essay questions of candidates taking its tenth written examination in November 1958:

William P. Albaugh  
Robert M. Allen  
Dorothy V. Anderson  
Stewart B. Axtell  
Arthur W. Ayers  
Donald E. Baier  
Joseph E. Barmack  
Robert P. Barrell  
Harold Basowitz  
Roger M. Bellows  
Hedda Bolgar  
Katherine P. Bradway  
Roy Brener  
Joseph E. Brewer  
J. F. T. Bugental  
Bettye M. Caldwell  
Robert Callahan  
Aaron H. Canter  
Hilding B. Carlson  
Abraham Carp  
Robert C. Challman  
Jacob Cohen  
Rex M. Collier  
George E. Copple  
S. Thomas Cummings  
Henry P. David  
Joseph G. Dawson  
Gordon F. Derner  
Solomon Diamond  
Robert D. Dugan  
Donald Ehrman  
Jerome H. Ely  
Leonard D. Eron  
Edmond F. Erwin  
Norman L. Farberow  
Herman Feifel  
Jerome Fisher  
Frank M. Fletcher  
Sol L. Garfield  
Leonard D. Goodstein  
Phillip A. Goodwin  
Harold A. Goolishian  
Mary E. Grier  
Stephen Habbe  
Ray C. Hackman  
Gerard Haigh

Julia C. Hall  
Ward C. Halstead  
Eugenia Hanfmann  
A. Arthur Hartman  
Starke R. Hathaway  
Ralph W. Heine  
Leo A. Hellmer  
Joseph C. Heston  
Jules D. Holzberg  
H. Elston Hooper  
Albert L. Hunsicker  
Marvin Hyman  
Ira Iscoe  
Alexander Jasnow  
Edith Joelson  
Tatania Juzak  
Marvin W. Kahn  
Raymond A. Katzell  
George A. Kelly  
Seymour G. Klebanoff  
Walter G. Klopfer  
Irwin J. Knopf  
Frank J. Kobler  
Carl G. Lauterbach  
Louis Levine  
Wallace V. Lockwood  
Louis Long  
Robert L. McFarland  
Laurence S. McLaughran  
Herbert B. Malos  
Leslie F. Malpass  
John B. Marks  
Joseph D. Matarazzo  
Robert H. Mathewson  
Christine M. Miller  
Robert S. Morrow  
C. Scott Moss  
O. Hobart Mowrer  
Julian Pathman  
Frank A. Pattie  
Leon A. Pennington  
Keith J. Perkins  
Karl E. Pottharst  
Ernst Prelinger  
Roderick W. Pugh  
Edward T. Raney

Evelyn Raskin  
Harold R. Renaud  
Thomas W. Richards  
Margaret M. Riggs  
Milton L. Rock  
Alexander C. Rosen  
Alan K. Rosenwald  
Alan O. Ross  
John W. M. Rothney  
Floyd L. Ruch  
Edward A. Rundquist  
Joseph Samler  
William Schofield  
Audrey S. Schumacher  
C. Winfield Scott  
Winifred S. Scott  
A. Eugene Shapiro  
Marie P. Skodak

Sydney R. Smith  
William U. Snyder  
William Soskin  
John M. Stalnaker  
Bernard Steinzor  
Joseph Stubbins  
Norman D. Sundberg  
Erwin K. Taylor  
Kenneth S. Teel  
Robert L. Thorndike  
Leona E. Tyler  
John M. Vayhinger  
Robert S. Waldrop  
Harold Wilensky  
Clarence L. Winder  
Robert D. Wirt  
Philip Zlatchin  
Herbert Zucker

The American Board for Psychological Services has increased its membership to seven by the election of Irwin Berg, of Louisiana State University; Theodore M. Newcomb, of the University of Michigan; and Gilbert Wrenn, of the University of Minnesota. Karl F. Heiser, of Glendale, Ohio, is the newly elected President of ABPS. Continuing members are Roy Brener, of the VA Hospital, Hines, Illinois; Wendell Dysinger, Secretary-Treasurer, of MacMurray College; and Nathan Kohn, Jr., Past President, of Nicholson-Kohn Associates, St. Louis, Missouri.

Edgar F. Borgatta will serve as a consultant for a new course on the use of medium- and large-scale data-processing equipment in social sciences research at New York University Graduate School of Arts and Science.

Members of APA recently elected to the Committee on Diagnostic Reading Tests are: Pablo Roca, of the Puerto Rico Department of Education, and Maurice Woolf, of Kansas State College. Agatha Townsend, Frances Triggs, and Fred Westover continue as members of the committee on unexpired terms.

Irving N. Hahn, Psychologist at the Lancaster Guidance Clinic, has also been appointed Professor of Psychology at Pennsylvania State University.

**Robert J. Howell**, on sabbatical leave from Brigham Young University, has been appointed to the staff of the Utah State Hospital as a Senior Psychologist.

Additions to the Human Resources Research Office staff are:

To the Training Methods Division in Washington, D. C., **Herbert Klions**, formerly at the University of Pittsburgh, and **Joseph Openshaw**, formerly at the University of Tennessee.

To the Air Defense Unit at Fort Bliss, Texas, **Harold Thorgerson**, formerly at the University of Marburg.

**Abraham Levitsky** has established a private practice and has been elected President of the Greater St. Louis Society of Clinical Hypnosis.

**Donald G. Livingston**, formerly with the Prudential Insurance Company, has joined the staff of Stevens Institute of Technology as Assistant Director.

The following new appointments have been made to the staff of the Department of Psychology at the University of Miami: **A. S. Luchins** as Professor of Psychology and **Benjamin H. Pubols** as Assistant Professor.

**William H. Meanor**, formerly with the Jones & Laughlin Steel Corporation, has been named Coordinator of Personnel Procurement at Allegheny Ludlum Steel Corporation, Pittsburgh, Pennsylvania.

**Clayton A. Morgan**, formerly with the Texas Vocational Rehabilitation Division, has been appointed Coordinator of a Vocational Rehabilitation Counselor Training Program being initiated at Oklahoma State University, Stillwater, Oklahoma.

**Jerome H. Nagel** has formed Jerome H. Nagel Associates, consultants to management, in Brooklyn, New York.

**Jack Arbit**, **Bernard Aronov**, **Wan-Ho Chao Lee**, **J. Joseph Levin**, and **John Reisman** have been appointed Instructors in the Division of Psychology, Department of Neurology and Psychiatry, Northwestern University Medical School, Chicago. Aronov and Reisman also hold joint appointments

in the Children's Memorial Hospital; **Jack Arbit** has a joint appointment in the VA Research Hospital.

International Business Machines Corporation has announced the appointment of **Howard J. Page** to the position of staff engineer in the Endicott Product Development Laboratory, New York.

**Carroll C. Pratt**, of Princeton University, will be Visiting Professor at the University of California in Berkeley during this semester.

**Jack Shaffer**, formerly at the Columbus Receiving Hospital for Children, is now Assistant Professor of Psychology at Humboldt State College, Arcata, California.

**Rohrer, Hibler & Replogle** announces the appointment of **Howard P. Smith** to the staff of the Toronto office.

The following personnel changes have occurred in Psychology Services, Department of Medicine and Surgery, Veterans Administration:

**Eli Alson**, a graduate of the VA Psychology Training Program, University of Buffalo, has been appointed to the Psychology Service, VA Hospital, Lyons, New Jersey.

**Lawrence J. Bookbinder**, a graduate of the VA Psychology Training Program, Northwestern University, has been appointed to the Psychology Staff, VA Hospital, Ann Arbor, Michigan.

**Rayman W. Bortner** has transferred from the Fayetteville VA Hospital to the Psychology Research Project in Aging, VA Center, Kecoughtan, Virginia.

**George C. Hall** has transferred from the Murfreesboro VA Hospital to the Psychology Staff, VA Hospital, Brockton, Massachusetts.

**Edith Hinds** has been appointed to the Psychology Staff, VA Mental Hygiene Clinic, New York, New York.

**Edward J. Kelty**, a graduate of the VA Psychology Training Program, Duke University, has been appointed to the Psychology Service, VA Hospital, Montrose, New York.

**Marie Mabry** has transferred from the Murfreesboro VA Hospital to the Psychology Staff, VA Hospital, Brockton, Massachusetts.

**James E. Madden**, a graduate of the VA Psychology Training Program, University of Kentucky, has been appointed to the Psychology Staff, VA Hospital, Chillicothe, Ohio.

**Joseph J. Moylan** has been appointed to the Psychology Service, VA Center, Dayton, Ohio.



**Vladimir Pishkin**, a graduate of the VA Psychology Training Program, University of Utah, has been appointed to the Psychology Service, VA Hospital, Tomah, Wisconsin.

**W. Neil Shelton** has resigned from the Psychology Service, VA Hospital, Memphis, Tennessee.

**Shalom Vineberg** has resigned from the Oteen VA Hospital to accept a teaching appointment at Vanderbilt University.

**Alan Willoughby**, a graduate of the VA Psychology Training Program, University of Connecticut, has been appointed to the Psychology Staff, VA Center, Togus, Maine.

**Irving A. Woods**, formerly in the Personnel Research Branch of the Department of Army, has accepted a position as Chief of the Experimental Section, Human Engineering Laboratory, Aberdeen Proving Ground, Maryland.

**Robert E. Wurtz** has resigned from the Island Trees High School to take a position as Assistant Professor in the College of Education, Wayne State University.

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The Division of Biological and Medical Sciences of the **National Science Foundation** announces that the next closing date for receipt of basic research proposals in the life sciences is May 15, 1959. Proposals received prior to that date will be reviewed at the summer meetings of the foundation's advisory panels, and disposition will be made approximately four months following the closing date. Proposals received after the May 15, 1959 closing date will be reviewed following the fall closing date of September 15, 1959. Inquiries should be addressed to: National Science Foundation; Washington 25, D. C.

Postdoctoral fellowships in clinical psychology are available in the Division of Psychology, Department of Neurology and Psychiatry, **Northwestern University Medical School**, Chicago. These fellowships carry a stipend of \$2,400 or more. Individuals interested in applying should write to: Vin Rosenthal; Northwestern University Medical School, Department of Neurology and Psychiatry; 303 East Chicago Avenue; Chicago 11, Illinois.

The aim of the Research Fellowship Program of the **Southeast Asia Treaty Organization** is to encourage the study and research of such social, economic, political, cultural, scientific, and educa-

tional problems as give insight into the present needs and future development of the treaty area, viewed against a background of SEATO objectives. A limited number of advanced research fellowships is offered for 1959-60 to candidates from member states. Candidates will be selected on the basis of their special aptitude and experience for carrying through a major project of research. Preliminary screening of American candidates will be by the Committee on International Exchange of Persons of the Conference Board of Associated Research Councils. A grant will provide a monthly allowance of \$400 and tourist-class return air travel to the country or countries of research. Grants may be authorized for periods of from four to ten months. Applications should be submitted no later than April 15, 1959. Application forms and additional information may be obtained from: Conference Board of Associated Research Councils, Committee on International Exchange of Persons; 2101 Constitution Avenue; Washington 25, D. C.

Under the auspices of a grant from the National Institutes of Health, the Inter-University Council of the **Training Institute in Social Gerontology** announces 40 faculty fellowships for the second Summer Training Institute in Social Gerontology to be held August 3-28, 1959 in Berkeley, California. The purpose of the institute is to provide intensive training in order to increase the number of university faculty trained to teach and carry on research in the new and rapidly expanding area of gerontology. Fellowships are open to college faculty trained in one of the social sciences or related professional fields. Fellows will receive an award of \$500. In addition, the institute will defray transportation costs and provide living accommodations. For applications and further information, write to: Wilma Donahue; Director, Institute for Social Gerontology; 1510 Rackham Building, University of Michigan; Ann Arbor, Michigan.

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The United States Commissioner of Education has announced approval of the first **graduate fellowship programs** under the National Defense Education Act. These programs will provide 160 fellowships during the 1959-60 academic year at a total federal cost of approximately \$400,000, the amount thus far appropriated for this purpose.

During the period November 1, 1956 to October 31, 1958, grants by the **Grant Foundation** (130 East 59 Street; New York 22, New York) included:

**Bryn Mawr College**, for a follow-up study of subjects in the Student Council Study of Normal Personality

The **Children's Village**, Dobbs Ferry, New York, for a group therapy project with delinquent boys in a residential setting

**Harvard University School of Public Health**, for investigations on the health, growth, and development of school children in Italy and a group of similar ethnic origin in the United States to determine the effects of environmental influences

**Youth Consultation Service**, for testing the effectiveness of casework and group therapy treatment on teen-age girls

**Swarthmore College**, for an experimental course in emotional and motivational processes

**American Psychiatric Association**, for preparation and publication of a source book of scientific researches and publications in the field of youth development

**State University of Iowa**, contribution to the study of educational achievement test data and school grades in the prediction of college success

**Teachers College, Columbia University**, supplementary financing of a study of the prediction of later occupational histories and success

Ida S. Hill, of Utah State Hospital, has received a one-year renewal of a research grant from the National Institute of Mental Health to continue studies in the area of group dynamics.

The United States Office of Vocational Rehabilitation has awarded grants:

To the **Woods Schools**, Langhorne, Pennsylvania, for a research project to determine points of view regarding the capabilities and disabilities of mentally retarded young adults

To **Southern Illinois University**, through the American Association for the Blind, for a training program to prepare rehabilitation counselors for the blind

The United Cerebral Palsy Association has awarded a grant to the **Speech Department** of the University of Pittsburgh to strengthen its research and training services. The grant will be administered by Jack Matthews.

The Institute of Mathematical Statistics and the University of Chicago have established a series of publications entitled **Statistical Research Monographs**. The primary purpose of this series is to provide a medium of publication for material of interest to statisticians that is not ordinarily provided for by existing media. *Authors are invited to send manuscripts and*

correspondence concerning the series to: Leo A. Goodman; Department of Statistics, University of Chicago; Chicago 37, Illinois.

The Department of Psychology of the Southeast Louisiana Hospital held its second Annual Symposium, February 11-13, 1959, on "**The Training of Psychotherapists**." This program is being supported by USPHS funds in the form of a training grant.

The Council of Psychoanalytic Psychotherapists (924 West End Avenue; New York 25, New York) held a symposium, November 8, 1958, on "**Termination in Psychoanalysis**."

William H. Brown, of the Salt Lake County Hospital, Utah, conducted a **mental health workshop** on February 19-21, 1959 sponsored by the Cheyenne Mental Health Association.

The Annual Convention of the **American Psychiatric Association** will be held in Philadelphia, Pennsylvania, on April 26-May 1, 1959. For further information, write to: Central Office, American Psychiatric Association; 1700 Eighteenth Street, N.W.; Washington 9, D. C.

The seventh Annual Conference of the **American Society of Adlerian Psychology** will be held on May 2-3, 1959 in New York City. Further information and detailed program may be obtained from: Irvin Neufeld; 103 East 86 Street; New York 28, New York.

The second **Caribbean Mental Health Conference** will be held in St. Thomas, Virgin Islands, on April 10-16, 1959. For further information, write to: Edwin A. Weinstein; Chairman, Program Committee, Second Caribbean Conference for Mental Health; St. Thomas, Virgin Islands, USA. It is requested that members of the **International Council of Women Psychologists** who wish to go, notify the ICWP President: Dorothea Ewers; 745 Aberdeen Drive; Crete, Illinois.

The Annual Convention of the **National Association for Retarded Children** will be held in Cincinnati, Ohio, on October 21-24, 1959. For information, write to: NARC Convention; 99 University Place; New York 3, New York.

On May 19-22, 1959, Dunlap and Associates, Inc. (429 Atlantic Street; Stamford, Connecticut) will present at Stamford, Connecticut, its seventh **Annual Human Engineering Institute** dealing

with human capabilities and limitations as they relate to the design of complex man-machine systems, specific equipment items, consumer products, and workplaces.

Opportunity to visit psychological research institutes in England, Sweden, Soviet Russia, and Czechoslovakia will be provided during a **Traveling Seminar**, arranged by Teachers College, Columbia University, July 5–August 19, 1959. For further information, write to: Goodwin Watson; Teachers College, Columbia University; New York 27, New York.

The **Virginia Beyer Memorial Lecture** will be given by Samuel Novey at Springfield State Hospital (Sykesville, Maryland) on April 17, 1959. The topic will be "Considerations on Religion in Relation to Psychoanalysis and Psychotherapy."

The **Walter Van Dyke Bingham Lecture** will be given by J. P. Guilford at Stanford University on April 13, 1959. The topic will be "Three Faces of Intellect."

The Society for the Scientific Study of Religion (Hartford Seminary Foundation; Hartford 5, Connecticut) is sponsoring a program "New Perspectives on Man for use in the **Empirical Study of Religion**" at the University of Chicago on April 10–11, 1959.

The **National Education Association** (1201 Sixteenth Street, N.W.; Washington 6, D. C.) announces the National Training Laboratory in Group Development and the National Training Laboratory for Educational Leaders to be held in Bethel, Maine, this coming summer.

The Committee on Diagnostic Reading Tests announces **Work-Conferences in Reading** during the summer 1959. For further information, write to: Frances Triggs; Kingscote Apt. 3G; 319 West 119 Street; New York 27, New York.

Arizona State University will hold a postdoctoral workshop in clinical psychology on March 26–28, 1959. The theme will be "**A Phenomenological Approach in Clinical Psychology**." Co-Directors of the conference are Sydney Smith and Gerard Haigh.

In the summer of 1959, the University of Michigan will sponsor two National Science Foundation programs for college and junior college teachers of psychology. The first program will enable each of ten college teachers to spend the summer working on research of his choice with a member of the Michigan staff. Participants may either begin research which they propose to continue on their own campus, carry out research which they can complete during the summer, or participate in an ongoing research program. The second program is a conference, June 22–July 10, 1959, in which 15 college teachers will participate in seminars designed to review recent developments in general and experimental psychology and in methodology. Participants in both programs will receive traveling expenses and stipends of \$75.00 a week. Further information on these programs may be obtained from: W. J. McKeachie; Department of Psychology, University of Michigan; Ann Arbor, Michigan.

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## Convention Calendar

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**American Psychological Association: September 3-9, 1959; Cincinnati, Ohio**

*For information, write to:*

Roderick H. Bare  
American Psychological Association  
1333 Sixteenth Street, N.W.  
Washington 6, D. C.

**Southern Society for Philosophy and Psychology: March 27-28, 1959; St. Louis, Missouri**

*For information, write to:*

Wilse B. Webb  
Department of Psychology  
University of Florida  
Gainesville, Florida

**Eastern Psychological Association: April 3-4, 1959; Atlantic City, New Jersey**

*For information, write to:*

Carl H. Rush  
P. O. Box 252  
Glenbrook, Connecticut

**Southwestern Psychological Association: April 16-18, 1959; Topeka, Kansas**

*For information, write to:*

Beatrix Cobb  
Texas Technological College  
Lubbock, Texas

**Western Psychological Association: April 16-18, 1959; San Diego, California**

*For information, write to:*

Ivan N. McCollom  
San Diego State College  
San Diego 15, California

**Southeastern Psychological Association: April 23-25, 1959; St. Augustine, Florida**

*For information, write to:*

Susan W. Gray  
Box 232  
George Peabody College  
Nashville 12, Tennessee

**Midwestern Psychological Association: May 7-9, 1959; Chicago, Illinois**

*For information, write to:*

I. E. Farber, Secretary-Treasurer  
Midwestern Psychological Association  
Department of Psychology  
State University of Iowa  
Iowa City, Iowa

**Rocky Mountain Psychological Association: May 14-17, 1959; Sun Valley, Idaho**

*For information, write to:*

William H. Brown  
Department of Psychiatry  
University of Utah College of Medicine  
156 Westminster Avenue  
Salt Lake City 15, Utah

**Inter-Society Color Council: April 1, 1959; New York, New York**

*For information, write to:*

Ralph M. Evans, Secretary  
Inter-Society Color Council  
Color Technology Division, Building 65  
Eastman Kodak Company  
Rochester 4, New York



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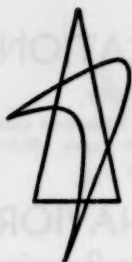
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